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ON THE RELATION OF NEUROLOGY TO PSYCHOLOGY.

BY HENRY H. DONALDSON, PH. D.

In the following pages it is proposed to make some statements regarding the relation of neurology to psychology as viewed from the standpoint of the former, and also to give a résumé of some rather recent neurological investigations.

Anatomical results have a reputation for superior credibility, and it is a generally accepted idea that within the limits of gross anatomy this reputation is well grounded; but when we glance at the work in minute anatomy or histology, it seems as though a long time must elapse before this latter would be thus honored. The field is clearing, however, and many existing contradictions are rather apparent than real.

As the result of a long discussion in his last paper, v. Gudden[1] nevertheless reaches the conclusion: "First of all let us have anatomy, or if physiology must come first, then never without anatomical control." In the

study of the form and functions of the nervous system there is reason, then, for accepting the anatomical data—though they are far from perfect—as the norm, and for measuring physiological results by them. It follows from this that the more accurate the standard the surer the progress.

The anatomy of the central nervous system is to-day largely topographical. The units are groups of cells and bundles of fibres, and the question to be answered is, as a rule, how these are united. The methods employed to determine the correlation of function with form are the association of the failure of some function with the failure of some part, or the development of the function with the development of the part. At the outset this investigation is balked both from the clinical and experimental sides by those cases where loss of nervous substance is not apparently associated with any or with a corresponding functional disturbance; but these cases, anomalous as they at first appear, will without doubt become clear under proper anatomical investigation, coupled, as it must be, with a much finer analysis of function than is now in use. It is in this direction that much is to be hoped from the improvement of psycho-physical methods.

Leaving those experiments which have localization as their aim, there arises a further question. Given a group of cells with a definite function, what is the fundamental difference between this group and others with a different function? This leads to the much less worked, and as yet less fruitful, field in neurology—the characteristics of the cell itself.

The first steps here are the study of the form and chemistry of the cell. We need not stop at this point, however, but at once go on to ask how far we may

reasonably hope to carry this investigation—to inquire what aid psychology is to expect from neurology in ultimate analysis, and whether it will ever come to pass that from the morphological characteristics of the cell its function can be inferred. It must be admitted that thus far the attempts to infer even from the general form of the brain to the grade of intelligence have failed, and yet that appears to be one of the least difficult questions that wait for an answer. It may therefore be asked whether, without the hope of being able to solve any world-riddle, with failures in correlation in full view, is it worth while to pursue neurology as an aid to psychology? An answer based on the following facts may be given in the affirmative. Neurology is the common factor of most of the approaches to psychology. The comparative, the developmental, the anthropological, the morbid, and the experimental methods all have it as a point of departure, and it may fairly be assumed that further anatomical knowledge will give us an insight into the working of the central nervous system far deeper than that which we now possess; that we may learn to associate certain cell changes with the exhibition of certain functions, and that at present we have no reason to fix a limit beyond which the method may not be carried. Accurate neural anatomy must therefore go hand in hand with accurate psychological analysis.

An account of some recent observations in this field, though fragmentary, may serve to illustrate the tendency of investigation in it. The contributions come from various sources. The anatomists, the physiologists, the psychiatrists, and the morphologists are all concerned. The physiological work has thus far been crippled by the lack of anatomical control; the pro-

fessed anatomists and morphologists have but partially specialized in this direction, so that most of the detailed studies have come from the psychiatrists, men who have other engrossing calls on their time, and whose anatomical work must therefore suffer from discontinuity.

The studies on the physiology of the frog's brain by Steiner,[2] of Heidelberg, which appeared in 1885, mark a distinct advance in the physiological methods. They are careful and accurate, and he sharply distinguishes between facts and theories. The chief technical aim of this investigator was the highest experimental accuracy and the reduction of the non-conforming cases to zero. In this he has succeeded admirably. A similar series of observations on fish,[3] some preliminary account of which has already been published, will probably appear soon. Through these results it is evident that the idea of the development of function has gotten a hold on physiological work. A mammal can be kept alive but a short time without its cerebral hemispheres, while Steiner has shown that the fore brain can be removed from a bony fish and yet the latter moves about and feeds in apparently a normal manner. It is to be remembered that in the teleosts the mantle of the fore brain is a non-nervous sac. Is it to be concluded from this that in the bony fish the voluntary control is seated in some part of the brain other than the cerebral hemispheres, or that we have in these fish an animal that is mainly reflex, with a grain of voluntary control? Both ideas are probably correct. Reflex and voluntary centres have differentiated out of more generalized nervous matter, and as this fish has no functionally developed organ corresponding to the usual seat of the voluntary impulses,

it is conceivable that the lower centres of the brain which are alone functional have a more generalized activity, and that they may be the seat of the imperfect voluntary impulses which appear to exist in this animal. So that, looked at from one point of view, the animal is mainly reflex, that is, the voluntary impulses are quite subordinate, while looked at from the other point, such voluntary impulses as it does possess have their seat in sub-cerebral centres. We have, then, in the development of the nervous system in the animal series, not an evolution in the sense of Bonnet and the older naturalists, namely, differences of bulk, proportions remaining the same; but all through there is a most marked alteration in the relative development and physiological value of the different parts. It is an idea as old as Aristotle, that animals possess but one adequate means of defense, or, to put it more generally, have but one apparatus for doing a given thing. The activities of an animal are partly reflex and partly voluntary, but the proportions vary in each case. In passing from a fish to man we are struck by the increase in the voluntary control, and, according to the principle above stated, we should expect—what we find—a most notable decrease in reflex development. If that were not the case we should have in man the nervous system differentiated along two lines, that is, a double apparatus for the same thing, which approaches the impossible. This idea has its application to the recent work on localization. Without the visual centres in the cortex a man cannot see at all; a dog can perhaps see a trifle, a rabbit more, and so on. This fact receives its explanation in that, the more the functions are pushed into the cortex, the more the lower centres become dependent

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on it for their very existence, so that removal of the cortex is followed by the degeneration of these lower centres in man and the higher mammals, while, as we descend the animal series, these centres become less and less dependent on the cortex and better able to act alone, as in the frog and fish. Thus the ability to use the lower centres of vision alone appears inverse to the cortical development.

A justifiable complaint is made against the lack of anatomical control in the majority of the physiological experiments. This is due to the practical difficulties in making such a control, which are plain to any one who knows what control means. The matter has been of late slightly improved, both Munk and Goltz having turned over the brains of carefully observed animals to anatomists. But this at best is the sporadic exhibition of a virtue which should be systematic. It is in a high degree naïve for an operator to maintain that a given animal kept alive after operation has suffered a loss of brain substance which is fully represented by what was removed at the operation, and then to make this assumption the basis for further inference. In every case the secondary degenerations must be considered, and the laws of secondary degeneration from the cortex are not yet well enough known to permit in most cases a reliable inference from the injury alone. Even the brains of dogs operated on by Munk, where the operation is precise and elegant in the highest degree, show changes which can only be made out from sections.

To turn to the anatomical side. It is from the method of v. Gudden that the most is to be hoped at present, or it would be, perhaps, better to define it as the "experimental-anatomical method," for, strictly speak-

ing, that of v. Gudden implies the use of new-born or very young animals, and the results thus obtained differ somewhat from those gotten by experiment on adults. The gist of this method is the artificial development of atrophies. But the very means which is here used to unravel the course of the fibres in the centres is itself a legitimate object for experimentation. Far too little is known of the real significance of degeneration to make it a perfectly safe means by which to study other problems.

While speaking of methods we may mention that of Flechsig, or the method of tracing fibre bundles by the development of their medullary sheaths. The assumptions on which this proceeds are that the fibres forming a bundle physiologically distinct assume their medullary sheaths at one time, and that the fibres surrounding them, but physiologically different, do not get their sheaths at the same time. These assumptions are, however, largely open to investigation, and it would increase confidence in the results thus obtained if the details of the process were more clearly known.

As to anatomical results, those of v. Monakow[4] are specially important, from the care and deliberation which characterize the work. In rabbits he has demonstrated the representation of the cortex in the optic thalamus. When a portion of cortex is removed, a part of the thalamic ganglia atrophies, and the relation is a fixed one. So far as tested, similar relations hold for man also. The idea of such a representation had earlier been put forward by Luys, but without any experimental proof. This demonstration is very suggestive, for not only have we the notion of the representation of the cortex in the optic thalamus, but, as cells in the latter atrophy on the removal of the

cortex, they cannot, in mammals as low as rabbits, functionate alone, and must be regarded as intimately associated with the cortex.

General notions of much value have come from Gaskell's[5] attempt to bring the sympathetic system into line, and the introduction of some order and method into that portion of the nervous system has been a very solid contribution. At the same time the spinal ganglia, and those along the cerebral nerves and in the sympathetic, are still riddles, and while they remain without explanation, it may seem a trifle overhasty to rush on to the anatomy of such complicated structures as the brain and cord. No one doubts the value of comparative anatomy for this work, but it might be wise to extend the comparison line into the invertebrates. Dominated by the medical idea that the study of these things rapidly decreases in value as we pass from man downwards, and having a more or less defined feeling that the invertebrates were intended by Providence for the use of the morphologists alone, neurologists have until recently neglected this division; yet it is in the arthropoda that the widest divergence in sense development among nearly related forms is to be found, correlated with differences in the supra-oesophageal ganglia. This ganglion, in some ants, has a cortex, and in so far deserves to rank as a brain.

To those acquainted with histological literature the work of Golgi[6] is probably familiar. The essential point of his technical method is the staining of nerve cells with silver nitrate or mercuric chloride, while the surrounding substance remains unstained. The cell and its prolongations thus stand out like a silhouette. The cell structure is unfortunately lost by this treatment, but the prolongations are beautifully exhibited.

The notion of cell prolongations and their significance, previous to Golgi, may be stated as follows: These cells were considered to have one unbranched prolongation, the axis-cylinder process, which continued itself into a nerve fibre. The other, the so-called protoplasmic processes, were branched and were thought to anastomose. Golgi's preparations show first that the axis cylinder, which is wonderfully demonstrated, is always branched, and second, that the protoplasmic processes do not anastomose. Since they generally trend toward bloodvessels, Golgi looks upon them as nutritive arms.

To return to the axis-cylinder processes: These are not all alike. Two types are recognized, and the typical forms are perfectly distinct. The first is that in which the axis cylinder, maintaining its identity, gives off fine lateral branches. The second is that in which the branching is profuse and rapid and in a short space the identity of the axis cylinder is lost in a network. Cells of these types are found separated in the different parts of the brain. On examining the spinal cord it is found that the cells in the anterior cornua belong to the first type, while those in the posterior belong to the second. It was this observation which led Golgi to develop the theory that the cells of the first type were motor and those of the second sensory in function. For the moment accepting this view, there still remains to be explained the meaning of the branches from the axis cylinder and the manner in which the cells of the second type are joined with the periphery. Golgi thinks that it must be through the anastomoses of the branches from the axis cylinder that the several cells are put into communication with one another. All the branches from the axis cylinders

of the first type are thus used, but only a part of those of the second. When an entering fibre loses its medullary sheath it becomes essentially an axis cylinder, and by this method stains as such, so that fibres in this condition can be followed in the neighborhood of the cells. In the posterior cornua of the cord there are fibres which thus enter and break up into a network analogous to that of the axis cylinder of the cells of the second type. It is supposed that this network is in connection with the network of the cells, and that at this point the isolated conduction ceases and the fibre is connected not with one cell but with a group. According to this idea we have to deal with groups of cells instead of single ones, and the explaining power of such a view is considerable. It would have been scarcely worth while to dwell on these observations of Golgi if they were questionable. So far as his facts and plates go they are perfectly reliable, and he understands as clearly as others where the facts leave off and speculation commences. These results have gotten into our literature only somewhat recently, and in order to show that the ideas have not grown up in a night, it may be mentioned that Golgi's first investigation with this method was published in 1874, and from that time up to 1886 he has been more or less occupied in these researches.

This method is selective. Not all the cells are stained; in fact, as a rule, only a small proportion is affected. If now the reaction is a chemical one, it is to be inferred that those cells which stain are in a peculiar chemical condition, and this suggests that changes in the nutritive state of brain might alter the manner in which it is stained.

In this connection at least a reference should be made to the modifications of the above method by

Mondino, a pupil of Golgi. He found that if sufficient time was allowed, whole human brains could be stained throughout by mercuric chloride. It is claimed that this will give a means of following out cell connections equal to any now used, but thus far it has not been put to the test.

It was necessary to wait until something had been said of the experimental method in anatomy and of Golgi's results before referring to the conclusions of Forel,[7] which involve both. Making use of two guinea pigs, he removed the facial nerve in one by pulling it out at the point where it emerges from the base of the brain, and in the other where it emerges from the skull he sectioned it. In the first animal the nucleus atrophied completely; in the second there was some atrophy, but many cells and fibres were simply shrunken, the difference between the two nerves and nuclei in the two animals being very marked. His conclusion from this is that the cell and its fibre considered as one organism can, like any other individual, lose a part of itself without being killed or seriously disabled, but as the relative size of the part removed becomes greater, the injury to the remaining part becomes more marked, until finally, if enough be removed, the remainder will die. According to this view, when the facialis has been separated at the base of the brain, enough has been removed to kill the remaining part, but when it is separated at the point of emergence from the skull, the part removed is sufficient to cause only a severe disturbance in the fibres and cells, and not their death. As is plain, this is rather a suggestion than an explanation; but it is a valuable one. Here is an attempt to give some meaning to the "nutritive centre" theory and the notions of degeneration involved in it.

To return for a moment to the fibres ending in a network, which Golgi finds in the posterior cornua of the cord. Most probably, according to Forel, these fibres arise from peripheral cells, and the network represents the termination in the cord of a fibre which has travelled from a peripheral cell through the posterior roots to the cord. But this is analogous to the cell of the first type, which sends the main stem of its axis-cylinder process directly by a fibre to a muscle, there to end in a network. The picture that we have then, in its simplest form, is that of a central or motor cell sending its axis cylinder toward the periphery and there forming a network, while the peripheral sensory cell sends its axis cylinder toward the centre and there likewise forms a network. Both sensory and motor cells are then of Golgi's first type. But there are still the cells of the second type left over—cells which may be conceived of as modified from the first by the excessive shortening of that part of the axis cylinder which would represent the nerve, thus bringing the terminal network close to the body of the cell. These need to be accounted for. If Forel's idea is correct they are to be designated as central cells, the function being indeterminate. Their relation to the fibres is further borne out by the way they react when the nerve is sectioned. It is known that the disturbance caused in a nucleus by the removal of a sensory nerve differs from that following the removal of a motor nerve. In the former case it is rather a shrinking than a true degeneration of the cells which occurs. This is somewhat explained by supposing the connection with the fibres to be that assumed by Golgi and Forel, or at least by supposing the manner of connection in the two cases to be different. Objections are not wanting

to these views, but at the same time they are supported by the recent observations of His[8] on the human embryo, where the developing sensory cells in the spinal ganglia send their processes in toward the centre.

The above summarizes, in a way confessedly incomplete, certain recent advances in neurology, with a view to indicating what the field is and what some of the results are. If the idea be admitted that pyschic activity is conditioned by anatomical structure, then these results have a significance for psychology.

AUTHORS CITED.

(1) v. GUDDEN.—*Zeitschrift für Psychiatrie*, Bd. 42, 1886.

(2) STEINER.—*Physiologie des Froschhirns*, 1885.

(3) STEINER.—*Sitzungsber. d. Akad. d. Wissen.*, Berlin, 1886.

(4) v. MONAKOW.—*Archiv für Psychiatrie*, Bd. XII.

(5) GASKELL.—*Journal of Physiology*, Vol. VII, No. 1, 1886.

(6) GOLGI.—*Sulla fina anatomia degli organi centrali del sistema nervoso*, 1885.

(7) FOREL.—*Archiv für Psychiatrie*, Bd. XVIII, H I.; 1887.

(8) HIS.—*Abhandlungen der Königl.-Sächsischen Gesellschaft der Wissenschaften*, Bd. XIII, 1886.

INSISTENT AND FIXED IDEAS.

BY EDWARD COWLES, M. D.

The student of introspective psychology must assume a standard of cerebral and mental health as the basis of his study. It must be sought in the healthy mind in a healthy brain; these are the inseparable subjects of the inquiry.

The student of insanity must assume, in like manner, the same standard of soundness for that which is insane, and he must study it in the terms of psychology; and the physiological basis is definite enough, if the communications with it are maintained, to permit safe and even bold excursions among the dangers of error that beset the path of the purely intuitional school. With practical reference to such a standard, modern psychiatry has the credit of having differentiated two general groups of idiopathic mental disorders. The first group includes those that may happen to any healthy mind in a healthy brain, and are manifested in typical and regular forms and courses of mental phenomena. The other contains those that occur in unstable minds correlative to constitutional brain defect—hereditary or acquired—and that are manifested in more or less irregular forms in which the phenomena are made distinctive by being modified and varied in their order and degree from those in the first group. The term "ordinary insanity" has been

applied to the disorders that spring from a relatively sound physical basis. The modern use of the term "paranoia" has been gradually enlarged till it tends to include all manifestations of hereditary and acquired chronic instability of mind. Thus there may be "ordinary insanity" in relation to the healthy brain, and "paranoia" in the unstable mind and defective brain. The general relation of these physical and psychical elements may thus be stated in terms of function: as the stable mind is to the unstable one, so are the disorders—psychoses—incident to the former, to the degenerative psychoses in the latter—or so is ordinary insanity to paranoia.

While pathology and psycho-physics are striving to let in the light upon the mechanism of mental phenomena, the clinical student must not wait for their guidance. His work is as essential to the elucidation of the truth, and his data are definite, tangible, and constant enough to endow them with scientific values. The science of psycho-pathology deals directly with the central object itself; the mind is the man, and the conservation of mental integrity is the aim. In the minute study of the psychical elements involved the clinical student has in his field as important a branch of the new psychology as the pathologist or the psycho-physicist; all must work together in the solution of the problems.

The recent words of Sir James Paget, upon another subject, are as applicable here, that the sick-room is a laboratory with its crucial experiments as real as those in which "culture experiments" are instituted in bacteriology. Kraepelin, speaking of natural science as the great method in medicine, says that "only by the inner connection of brain pathology with psycho-

pathology can we succeed in finding the laws of the reciprocal relation between somatic and psychic disturbance, and thus get to a really deeper understanding of the phenomena of insanity."[1] Sir J. Crichton Browne strongly stated, in 1878, the absurdity of resting upon an intimate knowledge of brain-cells, or of the deviations from healthy mental states in which insanity consists. Advancement must be made on both these lines, which must converge and unite. "But," he also says, "in that particular branch of psychology that is conversant with morbid mental states, little or no work is being done in Great Britain. In the literature of insanity of to-day there is no attempt at mental analysis, and only a most perfunctory attempt at a classification of the expressions and products of the diseased mind."[2] Exner also, in his remarkable study of cerebral localization, found reason to complain of the inadequacy with which clinical symptoms are described in asylum reports.[3]

To a better purpose is Kandinsky's very full and exceptionally fine study of three cases of hallucinations. He says: "What is wanted, first of all, is a severe, accurate and detailed study of the phenomena of hallucinations, and but very few (three by Sander and one by Pick) have been studied with sufficient care.[4] In modern psychiatry there is an evident tendency to return to the study of psychical phenomena and to break away somewhat from the dicta of the cerebralists. Insanity, in its origin at least, may be

[1] Kraepelin, *Compendium der Psychiatrie*, 1883, p. 3.

[2] Presidential Address, British Medico-Psychological Association, *Journal of Mental Science*, Oct., 1878.

[3] Exner, *Localisation der Functionen*, 1881, *passim*.

[4] Kandinsky, *Kritische und Klinische Betrachtungen*, 1885, p. 2 *et seq.*

as much a matter of disorder of the mind as of disease of the brain.[1]

Accepting the proposition that every manifestation of mind is correlated to a definite mode and sphere of brain activity, and the aim being a convention of the two lines of study, the indications are plain to the alienist. Psycho-pathology demands, upon one of these lines, careful and detailed analyses of morbid psychical reactions. This article is an attempt to comply with that demand.

From the clinical point of view the student finds himself in a region of ever-widening interest and novelty, for so much of it is unexplored. In examining these products, some salient or eccentric growth brings to light or emphasizes common factors of the whole that would otherwise remain obscure. The infinite variety of the human mind is in nowise more plainly revealed than in its aberrations. In the whole range of its special powers or qualities they may become, singly or in groups, more conspicuous by their relative luxuriance or exaggeration, or by their absence or weakness and impairment. In the broad borderland of minor and partial aberrations there is an instructive field for study. At the points of departure from the normal states of mind is to be sought the genesis of mental disorders, and their nature is shown by the study of their origins. The nature also of the normal faculties themselves may be thus made clearer.

A most fertile source of such knowledge is to be found in that great group of limited disorders of ideation called "fixed ideas." In the understanding of these affections a great advance has been made within the last few years. They have been studied,

[1] Cf. Savage, *Insanity*, 1884, pp. 4, 21.

especially by the Germans, French, and Italians, during the last thirty years, and many names given to different varieties, but it remained for Tamburini[1] to show the common relation of these affections under the term *Zwangsvorstellungen*, first applied by Krafft-Ebing. They include the "metaphysical insanity," "insanity of doubting," and the many forms of "fears" of places and things.

Kandinsky mentions the difficulties of getting the intelligent co-operation of patients in his studies of hallucinations. The importance of the study of "imperative conceptions" is as great; and while its profit may be relatively much greater as touching earlier stages or less degrees of aberration, in these cases also the comparative integrity of the intelligence, except as specially involved, is a great aid. It is curiously possible to enucleate, as it were, certain well defined ideas and feelings, and to study their reactions between each other and the will.

The general characteristics of "the cases yet described" are thus briefly stated by Tamburini: "1st. That side by side with a fixed idea which is accompanied by fears more or less distressing, and an overpowering impulsion to certain acts, consciousness of the absurdity of such acts usually remains complete. 2d. That in all there is an almost absolute impotence of the will, not only to control the absurd ideas, but also an irrestrainable tendency to those acts. 3d. That in almost all the cases there was a very conspicuous hereditary predisposition to psychopathic disorders."

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[1] *Revista Sperimentale di Freniatria*, Fascicolo 1, 1883. Translated by Joseph Workman, M. D., Alienist and Neurologist, January, 1884.

A varied collection of cases of these affections has afforded the writer a study of great interest for a number of years. In these cases the relation seems to be clearly shown of these limited ideational disorders to primary delusional insanity, as indicated by Westphal's designation of them as "abortive monomania." Not only this, but in my observations there appears to be ample evidence that there may be every degree of development of these disorders, from the slightest departure from normal intellection to pronounced delusional conceptions. Also, that instead of passing in the typical way from stage to stage, the process may be arrested at any point in its progress and remain at a chronic stasis during the remainder of a lifetime. There is a multitude of the cases of the minor degree which, as far as they go, have all the essential characteristics of "fixed ideas," but in which the "idea" is not so "fixed" as to be always dominant; it can be resisted more or less successfully at will, in the milder cases, which are in a large majority. The ideas are simply insistent, and the term "insistent ideas" is here proposed as being the more generic, and as including all forms, some of which become "imperative" and "fixed." Here the words "idea" and "fixed idea" are distinctive, in contrast with "delusion" or "fixed and limited delusion." The one is not a belief, while the other has attained the pronounced degree of an insane belief.

Again, contrary to the conclusions of writers on the subject, it would appear that these affections are not "chiefly limited to a constitutional origin, especially to heredity." Here arises one of the most interesting questions. Primary systematized delusions so characterize paranoia, and so close is the relation

between them and fixed ideas or imperative conceptions, that these also are included in paranoia by many writers, with the implication that these affections always signify a constitutional origin in either acquired or hereditary defect. But degenerative psychoses are not manifestations of new powers acquired by morbidity in the organism: there is not more mind in the paranoiac brain. The criterion of insanity is not essentially the formulation of incongruous, "insane" conceptions — this is common enough in the sane mind and healthy brain,—witness dreams, the play of the imagination, etc. The inability to correct some of these, or to inhibit others, constitutes the insanity. Of imperative conceptions we should expect, *a priori*, to find that the conceptions are common enough to sound minds. But where, in the scale of increasing insistence, fixedness, or imperativeness, does evidence begin of their having a paranoiac basis of hereditary or acquired defect and mental instability? So, too, of systematized delusions; if but rare in a sound mind, this is enough to establish the premise.

Proper conservation demands at least a stay of inferences and implications in this matter. It might be expected to be nearer the truth to say that insistent and fixed ideas, especially in their milder and often "corrected" forms, are common to healthy minds and brains, or to those which have no more "acquired instability" or neurasthenia than would be argued as existing in a melancholia or mania of an ordinary insanity. As forming a symptom group of ideational disorders in the prodromal or abortive stages of ordinary insanity, they become, like delusions, emphasized in paranoia. Thus these affections being once

initiated, the gravest forms are simply more likely to be developed upon a basis of neurasthenia or the more radical paranoiac conditions; in other words, when there is hereditary or acquired nervous and mental instability.

In regard to other views that have been held of these affections, it does not seem necessary to delve deeply into the mysteries of "the inconscient" for an explanation of all these anomalies. Again, the common kinship of the recognized varieties of these affections, as shown by Tamburini, is finely demonstrated in a number of my cases, in which there have been developed, in succession in each individual case, several of the distinct forms. These transitions are not unusual, and it is probable that some of the forms now recognized as distinct, if it were possible to trace them to their origin, would be found to be secondary to the more common primitive ones.

It is not the present intention to attempt a demonstration of these propositions, which are thus broadly stated for the sake of indicating the nature and the interest of the questions that arise in these investigations. The limits of this article permit simply a detailed account of a case in which there is much to sustain the foregoing propositions. The unusual intelligence of the patient allowed a long series of mental phenomena, occupying years, to be traced even from their origin. At the outset the case was a common one of the "insanity of doubting," in which the idea was that simple acts were not right and it was necessary to repeat them to make them so. The complications that followed, and the stages of the malady through which the patient passed, came partly by logical evolution from the false premises, and partly from the growth of

a habit of methodizing thought and action; the latter was a characteristic process of circumvention of the limited imperative conception, against which the will was powerless for direct resistance. The processes of evasion were invented and systematized by a keen intelligence, and carried out by an otherwise efficient will, to the effect of accomplishing the desired purposes without directly antagonizing the fixed ideas. An interesting feature of the case is the unique combination of the characteristic "fear" of the patient for herself, with a "fear" in behalf of, and a "jealousy" against, another person, for whom she had both "love" and "admiration." The interplay and complication of these feelings are illustrations also of the probable fact that the abnormal association of "ideas" and their accompanying "feelings" may commonly enough involve in like manner other feelings than the "fears" of which so many varieties are named. This case with its extended details is here presented as a preliminary study of the subject. The medical "reporting" of such a personal history is justified by the proffered permission of this lady and by other proper consent.

Miss M. came under my care as an asylum patient at the age of 28 years. She was of a good family in heredity and otherwise; and both the parents were then living, and over 70 years of age. She was the youngest of ten children, all of whom had been healthy except two sisters who died of phthisis after marriage. The patient had a good physique, was a little above the average stature, and in good bodily health in all particulars. She soon established herself, in a company of lady patients of the better or convalescent class, as a person of more than ordinary intelligence

and good sense, and as usually amiable, pleasing and dignified in manner; though reticent, she was not unsocial. She was disposed, however, to dress very plainly and to be negligent in this regard, in a way that was inconsistent with her evident appreciation of what was pleasing and proper. Her intellect was, to ordinary observation, apparently unimpaired, and for a period of more than two years she revealed her morbid mental peculiarities to none but two of her physicians. She was well informed, read good books, chose the most intelligent persons as companions, could easily follow and take part in a psychological analysis of her own case, was keen of insight and quick at repartee. She was depressed at times and inclined to seclude herself in her room, and later she was sometimes irritable and sarcastic toward her associates; but in general she was known to them only as possessing the creditable characteristics above described. In brief, she was in many respects an interesting person.

To her physicians, however, she presented a medley of curious and inconsistent symptoms. She was admitted to the asylum as a case of suicidal melancholia, but as a "voluntary patient" (without a regular examination and commitment as an insane person). The immediate cause of her coming was a suicidal act. It is of special interest and importance in the diagnosis of some cases of primary disorders of intellection to differentiate them from those of the feelings characterizing true melancholia; this case strikingly illustrates the relation and contrast between these two forms of mental disease. To make this plain it is necessary to give an outline of this patient's history as it was received on her admission, and the earlier observations of the case before its riddle was solved.

There was no known neurotic heredity in the family history. She had always been physically strong and healthy, and never employed a physician. At about the age of nineteen years she first attracted attention to her mental disorder by secluding herself and refusing to see her friends. At about the age of twenty-five years her relatives became fully impressed that her condition was morbid and serious. She excluded them from her room, was difficult to manage, and spent her time in slovenly indolence.

Four months before becoming my patient she was brought to a neighboring institution for the treatment of nervous diseases. It was said that while there she was shy and silent, exercised no care of her clothing or person, neither working nor reading, lying on the bed if not watched, and all efforts to interest her in anything were failures. At times she was much agitated and felt that she was "going crazy" and said, "my proper place is in an insane asylum." She gradually became more depressed and suspicious, and entertained delusions of a gloomy character, of her own wickedness in relation to indiscretions of her early years, and of great wrong-doing. It was stated that "she developed the delusion that all who have anything to do with her would suffer injury or death; and that she made vows not to do certain things, and entertained the delusion that by breaking them she brought trouble, perhaps death, on a friend of hers." Finally, she got access to a bottle of laudanum and took "three or four teaspoonfuls," apparently with suicidal intent. "She manifested no remorse, but sorrow for her failure. When remonstrated with was sullen, and threatened to do it again at the first opportunity."

Two days afterward she was brought to the asylum.

Her intelligence, reasonableness and self-possession led to her being placed at once with the convalescent patients; and her manifest satisfaction with the change was significant in the light of later revelations. Her general demeanor was as has been described, but she was carefully watched, never allowed to seclude herself, and by night as well as by day the door of her room was kept partly open. There were signs of mental depression at times, and she was very reticent at first; but soon she began to repeat to her physicians the self-accusation before mentioned, and later told a story of having attempted homicidal acts, and alluded to other events of her history which she regarded as evidences of depravity and loss of conscience; this was not consistent with her daily, lady-like conduct. She spoke of herself as being insane, but at last discovered that she was unlike other patients who had melancholia. Indeed, the notable absence of the characteristic, underlying depression of feelings piqued inquiry. Her keenness of intellect and bright-witted though respectful rejection of the customary comforting assurances of possible recovery were entertaining as well as baffling. The assemblage of symptoms seemed anomalous. She at length said to me, "I have a monomania," and gave me more fully than before a curious, puzzling account of a long-existing fear that her ordinary acts might do harm to a lady friend whom she had always loved and admired, and whom she said she had once maliciously attempted to injure; and she manifested genuine grief about it. She readily understood and gladly accepted the explanation of the absurdity of these and other ideas, and tried to act upon this view of them. The characteristic qualities of a delusion appeared to be absent, and the depres-

sion to be secondary, or, as Krafft-Ebing says, "proceeding from the sad consciousness of the formal disorder of ideation—almost always painful and sometimes dangerous."

Soon after admission she began to insist that her previous suicidal act was not done with that intent, but gave a halting explanation of it. She finally overcame her reticence enough to give an intelligible and undoubtedly truthful account of the affair and its motive, essentially as follows: It appears that her intense pride had for years kept her from revealing the secret of her mental troubles; and the only exception to this was an intimation to her mother on one occasion. It was a new experience to have a sympathizing physician, and it became a source of comfort to her. Into her self-inquisition there finally came the idea of some impropriety, on her part, in finding the physician's visits to be agreeable; and upon this as a basis of self-accusation she finally determined to escape from the perfectly proper relation of which she morbidly thought herself ashamed. She thought she ought to be placed among the insane, and so conceived the plan of feigning the suicidal act, suggested by the opportunity that offered itself. She purposely limited the dose of laudanum to "a moderate quantity, and chose a time when its effect would be discovered in season to prevent serious consequences." She thought she would be sent to an asylum, and so conducted herself after the event as to insure this result. All this being regarded as evidence of absence of true suicidal feeling and motive, it formed an important factor in determining the subsequent treatment of the case as not being one of true melancholia.

She gave vague and partial accounts of many years

of mental pain, declared her hopelessness of recovery, and that death offered the only possible relief. Looking significantly about her room for points of suspension, she would say to me that she could not do it here, but if it were possible she would end her troubles. The consequent surveillance became annoying to her, and she begged to have her chamber door closed at night. When asked to promise that she would not attempt self-injury, she acknowledged the rightfulness of the request, but said, "You cannot trust me, my pledge is worth nothing; I have no conscience, and cannot trust myself; my life is ruined, and I justify myself in always gratifying my wicked desires regardless of right or wrong." She was told that she was mistaken, that she showed more than ordinary keenness of conscience, and that it was only a morbid sensitiveness of it that prompted unjust and undeserved self-reproach; that her sense of honor was so strong that I knew she could not betray my confidence, that she would be trusted, and must make the promise. The promise was made, her door closed at night, and the watching largely abated.

New information now received from her relatives gave an interesting insight into her real character. She was described by her teacher, when at the age of ten or eleven years, as the brightest of a large class of girls, and remarkably conscientious, once refusing to wear a medal awarded to her, for the reason that she thought the honor belonged to another. "She was the most truthful child" the teacher "ever knew, the life of the school, intelligent, high-spirited, and beloved." She maintained these characteristics until the age of twenty-five years. She was, as a school girl, mentally superior to her associates, and distin-

guished for her truthfulness and unselfishness. Though naturally reticent as to her personal feelings, she was bright and companionable, esteemed and admired by her friends. This corroborates the estimate made of her even under the adverse conditions of an asylum.

All went well with her for several months under the enforced parole. She grew more cheerful, and less reticent as to the details of her previous history. Then one more incident occurred which is of interest, in the aspect of the case now under consideration, as to the significance of its bearing upon the question of melancholia. Five months after her admission, a suicidal accident happened in another part of the house, in my absence; my assistants were naturally anxious. Just then this patient made, to one of them, a remark, the subject of which, when occasionally mentioned to me, had been treated as tabooed—that is, the allusion to suicide, and that she thought there was "no way here." The revival of close watching was a natural consequence, and her offered promise "not to attempt it here" was not understood or accepted.

Aggrieved by this she made one of her self-imposed vows, which will be hereafter described as characteristic, that she would never make another promise. Upon an appeal to me, she agreed to the soundness and justice of the position that there should rightly be the guaranty of her pledge before the restoration of her privileges. She begged that the promise should be waived, for, she said, "You know how dreadfully I shall suffer if I break my vow, but if you insist, I must do it." With my practical belief in the true nature of her malady, she was told after some days of "considering" it, and setting forth my responsibility, that the nurse would be at once directed to restore her

former privileges as if she had again given her parole; but that if she accepted my renewed confidence, and still had it in her heart to betray it in any way, she had no right to let me commit myself to such a course. The reply was simply, "You are right, Doctor!" Her appreciation of this proof of confidence was such that from this time forth there was no more reticence with me, but she asked that none of her associates or nurses should know her secret. The subject of suicide was practically dropped. The analysis now to be given of the mental phenomena of the case was worked out, and our relations remained upon this trustful basis for two years. She became more cheerful, and even hopeful. By dint of hours of talking, the earlier and later incidents of her mental history were made clear, and such was her intelligence that it was possible to trace to its origin the train of evolution of her morbid ideation; also to unify, as parts of one process, the strange and apparently incomprehensible events of her life. Their consistency was also shown with a personal character of rare quality, which she was not only unconscious of possessing, but which she felt to be vicious, not as a matter of delusion, but from the intricate complication and confusion of her morbid intellection. Above all it revealed the extent to which, under the domination of a tyrannous, imperative conception, the obscuration of such qualities of character can go, and the pitiful havoc and unspeakable torment that can be created in a mind that remains intelligent and conscious of it all. The interest of the case lies largely in the clearness of the differentiation it was possible to make between the earlier and later ideational elements that made up the psychological enigma. In a characteristic way, the earlier ones either disappeared

or became obscured in the later complications, and it is rare that their growth can be so clearly traced as in this instance. The peculiar details already given are not so trivial as might appear, but are valuable as representative of the mental qualities that are to be studied in this case. The array of symptoms, at first presented, was quite typical of melancholia with suicidal impulse, and such a diagnosis was justifiable; but closer investigation made clear the distinctly ideational character of the disorder as being "the insanity of fixed ideas" in an advanced stage. The plan of management of the case was to instruct the patient with a thorough understanding of the nature of her malady and how to set up a counter habit of motive and conduct; she was urged to take a "new master" —the dictum of her physician as the antagonist of her "fixed idea." The results of her first efforts to do this and of the study of the case will be given in the form of a mental history of the patient. She left my care some time ago, but the narration is given as of the present time.

During her early years, from ten to twelve, she remembers that she was sometimes depressed and had fears of harm, even of death, happening to herself or her relatives, and with no reason for them. The word "trance" became painful to her, because she had heard this and kindred subjects much talked about; so that this word in particular suggested thoughts of herself or friends being buried alive. In her twelfth year she was nervous and ill, and in consequence was taken from school for six months. She vaguely remembers having, during these two years, the fear of harm in connection with doubts as to whether some simple acts were right. She had to repeat some such acts; but she

had become quite well of these morbid experiences before the age of fourteen years. In this year she survived a nearly fatal attack of typhoid fever and was a year or more in recovering from its consequences. It was during this convalescence that, for the first time, she confided to her mother her worries and troubles. It is necessary to make some allowance for her present disposition unduly to reproach herself, but she thinks that at that time there were two prominent features in her character: that she was at times depressed and conscience-smitten, and that she was always of a jealous disposition. She believes she overcame her reticence and revealed her feelings to her mother only because she was sick and weak. The next year, while still not strong, she resumed attendance at school. Being relatively backward, she studied very hard, but led in scholarship. Her daily journey to school was fatiguing, and there were irregularity of meals and study by night. During this year menstruation began. The feelings of hesitation in performing simple acts, like those of three years before, now reappeared. In walking, dressing or undressing, for example, she was obliged to repeat many such actions; she did this, however, only when unobserved, and then could not help it. Here was the genesis of a common form of fixed ideas,—the idea that the act performed is not right, the accompanying vague fear that some harm will follow if it is not made right, and the necessity of repeating the act to make it so. From this point all the rest follows in its morbid train.

One of her schoolmates, a distant relative, was a girl of her own age, called in this history C. She was beautiful in person, lovely in character, and destined to exercise an extraordinary influence upon the after-

life of the patient M. The latter was the intellectual superior, but became jealous, she says, of the attention C. received, although she admired and loved her very much. M. did not manifest this evil feeling in any way, but it gradually developed to one of strong hatred, or one that seemed to be such in her extreme conscientiousness. In connection with this feeling there were evolved thoughts of harmful things that might happen to C., and then of self-condemnation as if she were guilty of desiring them to happen. This seems to have begun in the natural feelings common to all conscientious persons. M. next questioned herself if she would not be tempted to do violence to C. should there be an opportunity. This became mixed with the repetition of her acts; there was first a feeling of vague fear if she did not repeat them; then there came to be associated with the insistent idea a definite feeling of fear of harmful consequences to C. if the morbid impulse of repetition were not obeyed; then for a time this fear was dispelled by compelling herself to repeat the special act a certain number of times, more or less on different occasions. This fear of injuring C. was harmonized with her so-called hatred of her, as it was not so much because of the pain of contemplating possible harm to C. as because of the suffering M. feared would be inflicted upon herself by her offended conscience, if any such harm as she thought of did happen to C. It would be as if her thinking of harm in connection with C. had made it happen to her, and M. were to blame for it. Up to this point the morbid process had advanced through several stages of evolution and complication. M. realized all through this that she would not be even morally responsible perhaps, should such harm happen, but she went on all the same, yield-

ing to the domination of the morbid idea, which her will was powerless to resist, and fearing the self-accusations of her conscience—both the actual and the possible.

After this there came the apprehension that something would happen to C., and M.'s mind became tormented with thoughts of her own probable sufferings from the blame of which she knew her conscience would accuse her. She soon became unable to read, or hear, or think of any painful thing, or of death, as happening to any human being, without feeling and fearing that it might happen and then was about to happen to C., and mentally anticipating her own consequent sufferings. Thus these processes kept her mind full of painful feelings. She found a way to allay them by forcing herself to think of some person known to her, other than C., whom she would mentally substitute for the latter, and whom she would set up in her mind, the moment the morbid idea came, to be the recipient of the imagined or suggested harm. This worked well for a while, but soon began to be refined upon. It became necessary to choose for the substituted person some one with many opposite characteristics to C.; for example, there had to be a difference in age, sometimes of sex, initials of name, color of eyes and hair, stature, distance as to residence from C., and at last peculiar requirements as to time, place, etc., etc., to an endless extent. Next it became necessary to have ready in mind a number of chosen persons, two, or three, or four of whom, as the case might be, must be thought of in a certain order, etc. After a while, thinking of these persons ceased to give mental relief and another set had to be chosen, to wear out in turn. The process was this :—if in reading or in conversation or in any way,

by direct suggestion, or by the law of association of ideas, the thought of personal harm to any one came into her mind, the thought of C. as suffering that harm came also. Before the mental action thus interrupted could proceed she must call up the mentally prepared substitutes and imagine them as possessed of all the prescribed characteristics, and in the certain order, etc., as having the harmful thing thought of inflicted upon them. This imagining relieved her, but at last it became necessary to wish it to happen to them, then by degrees to imprecate it upon them with increasing intensity of oaths and cursing, although it shocked her very much. She became very expert in this process of exorcising the fear the morbid idea would arouse; she could go on directly with the conversation, for example, and keep up appearances. Her pride in this regard, her perfect concealment of this whole matter from every one, was extraordinary and very characteristic of the malady. It was only after six years of suffering, and at the age of twenty-two, that she first revealed something of it to her mother.

The process of relieving her mind of the painful idea when suggested in any way not connected with any acts of her own has been described. On the other hand, in regard to these, the hesitating and repeating impulse also grew upon her, and finally crept into all her acts. It will be remembered that she feared they would cause some harm to C. if not done right. She could at first relieve herself from this state of doubting or hesitating in an act, by exerting indirectly the inhibitory influence of self-control over the morbid repetition impulse, as for example, by intellectually prescribing that she should repeat the partial or full accomplishment of the interrupted act a certain number

of times, more or less on different occasions. As this process became more complicated and difficult to conceal, her intense pride in maintaining appearances led to devices for abbreviation. If she fell into a state of indecision in the attempted doing of any act, and needed to compel action to avoid betrayal of herself, she could succeed by strongly vowing that she would do it, adding the stimulus of harm to C. as the self-imprecated penalty of failure; of course, this involved all the dreadful consequences to herself. To reinforce this process, also, vowing became swearing, and then came the necessity of the most fearful and blasphemous imprecations upon herself, to compel herself to do or not to do certain acts, then or thereafter, upon pain of inflicting injury upon her friend, and of all its dreadful consequences upon herself. All this was sufficiently complicated, but she regards these things as primary in the sequences of complication and mental entanglement that were afterwards evolved. These two things, however, have run through all the phases of the fifteen years' existence of the malady: "the binding of herself over," as she says, by vows and oaths in a complicated system of methodizing all her acts, and the process of substituting vicarious sufferers of every harmful thing thought of, in order to shield her friend, and secondarily herself, from the consequences of imaginary harm to her friend. There was self-accusation, however, just as if she had maliciously wished the harm upon her friend. But all this was her secret. There was indeed a skeleton in her mental closet, animated by the Satanic double of her own mind, that tyrannized over her conscience and thoughts with a world of intricate formulae of thinking and doing. She says she has lived two lives.

The conditions so far described were quite fully developed when she was eighteen years of age. She had become unable to study, and then left school; for two years she led a listless, aimless life, she says, with loss of her natural inclination for reading, acquiring accomplishments, etc. She maintained her social relations in every way, however, and was sufficiently cheerful and spirited in manner, except with members of her own family. New complications were then evolved. She began to be compelled to give up her dearest friends. An innocent remark made by one of them might suggest the painful association of ideas in M.'s mind; or she might meet at a friend's house some person against whom she had acquired an aversion through her morbid idea. To be rid of the distress the painful memory would cause on meeting her friend, or visiting her house again, she must be rid of such friends; thus, one by one, she had to avoid them. Otherwise, also, the morbid idea brought in its train many others, and these brought another set, and so on in a wide-spreading combination, the outcome of which would finally narrow down to imaginary harm to C. If M. tolerated her friend any longer, and thus necessarily all the bad company of harmful ideas, she felt that she became a party to the combination, and thus subject to blame by her conscience under the compulsion of the tyrannizing idea.

The obstructive influence of the morbid association of ideas interfered with all the minor acts of her daily life. It affected the taking of certain articles of food, or going to certain places; interdicted certain things, or permitted them on certain days and not on others. Colors, pictures and ornaments were banished from her room. She was fond of dress, but certain materials

and many colors could not be worn, although she most liked them. Certain days were tabooed for shopping, because they were anniversaries of painful events in her morbid calendar; and certain shops could not be entered. When the new dress was obtained, very likely it could not be worn because of some newly imagined danger of potential harm to her friend. Thus it became more and more difficult to keep up appearances among her well-dressed associates; and retaining all of her pride in this regard, she began to seclude herself. Later, this led to the neglectful personal habits before described. Acts relating to all these things were complicated or prevented by her vow. She would not reveal her troubles to her friends —in fact, was ashamed and afraid to do so.

Finally, her relatives were made anxious by her peculiarities, and after a scene she submitted to the visit of a physician, who was told something of her physical condition. She was taken away to the country and greatly improved in a short time. But while there she heard of the death of a young neighbor, whom she had mentally used in the substitution process of wishing he might die to save her friend. This was a great shock to her, and she says that if she had deliberately killed the young man she could not have felt worse. For a year after that her condition was distressing; she secluded herself, became despondent and irritable. A new phase of her mental condition now developed, and there was a reaction from the unquestioning submission to her compulsive idea, and the growth of the feeling of resentful desperation. When about twenty years old she was again taken away to the country, and without any warning, "as if the fates had willed it," she was

taken to visit at the home of her friend, C., where she spent three months. It was a dreadful experience; she felt that she must and did keep up appearances. All her former admiration of her friend's character and beauty revived, but with it of course came the antagonizing jealousy, and these opposing feelings went on together. Her friend was lovely, fortunate and happy; she felt herself to be the contrary, and managed to evolve the notion that her friend was somehow to blame for her own sufferings. But again, a pleasant conversation would dispel the evil feelings, and she would wonder how such terrible things could exist in her mind. She does not know how the thought of injuring her friend arose, but the habit of mental vacillation between doing and not doing took up the thought and made her miserable. She knew the place where a razor was kept. She condemns herself unsparingly for all that followed, but she sometimes thinks that she became involved in a mental obligation to put herself to the test of going as far as possible with the act of killing C. and stopping just short of really wounding her. One day, in a room that suited the purpose, she approached C. from behind and drew the razor close across her throat, greatly frightening her; but M. succeeded in laughing it off as a joke. After this there was, of course, new and real cause for self-reproach, and the feeling took possession of her that she was at heart a murderess, and that this was the result of her former wicked self-indulgence. Finally, after a year of this worry, she was forced to get a young friend to spend the night with her before whose throat, when asleep, she drew a razor. This cancelled, as it were, the former act.

She spent the three years following the age of twenty with friends in various places, being unwilling to

return home. Matters secretly went from bad to worse with her, and the hope of some outlet from her mental troubles grew less, but she preserved appearances fairly well. It was during this period that she made to her mother the partial confession already mentioned. A sister died, about whom she suffered great remorse because of some unkindness shown her a few years before, and she confessed her feelings to an older sister. This did her some good, but soon all was as before. During the next two years she gave up her social relations more and more, ceased attending church, making calls, etc., finally saw only two or three friends.

During these five years a number of the persons died whom she had used in the process of mental substitution for C.; this cut her off from her associations, if she had any, with the families of those persons, besides causing an increase of her unhappy feelings. She did some reading, was sometimes taken to places of amusement, and could laugh and be companionable, but this was always hard. She often wished that she might die, but the idea of suicide never suggested itself, except to excite wonder, quite in a normal way, that any one could have courage to commit the act. There was always the love of life and longing for freedom from her troubles. In the latter part of this period she remembers that sometimes when alone with a young, old, or weak person, the thought would seize her that she could kill that person, and that there was nothing to prevent it but her own weak will. It did not grow to be an impulse to do the act; it was only a thought of it. The morbid mental operations before described still went on, but became more systematized. Among a number of strange

experiences at this time, the following may be mentioned: On one occasion, in a public library, she read in one of Dickens' stories a graphic account of a murder by cutting the throat. This affected her very painfully, as if it described a crime of her own. She had no peace till she went again to the library, and from the same book had secretly torn the leaves upon which the murder was described and rended them into shreds. Again, she found in a newspaper an announcement of the death of a person of the same name as C. This so worked upon her mind that she had to select the name of a young lady friend, and adding a letter to it, "to protect herself," she wrote an obituary notice and had it published in the same paper. This caused a great deal of talk, but M. held her peace. On the principle of substitution this act relieved her mind of the suffering she had been enduring. It had been just as if C. had really died and by M.'s own hand—or what was the same thing, because of her own thoughts; she cancelled the idea by the vicarious sacrifice of the other person.

This brings her history to the age of twenty-five years, the time when her relatives recognized her morbid condition, as was stated in the account first obtained. By that time she had reduced herself to the practice of staying much at home and in her room. She did not leave the house for ten months, and again did not leave her room for a year. She became very neglectful of personal care, with all which that implies. At the beginning of this period she had much insomnia, demanded and obtained sedative medicines. These were increased in strength as they lost their effect, till a narcotic mixture was furnished her which she took at will. She "would have it, and did have

it!" She took stimulants also. It can only be learned that the medicine was given at the suggestion of the druggist who prepared it, and was a "preparation of valerian and other strong nervine remedies." It was probably not so potent as she supposed, but the family physician, whom she utterly refused to see, thought "too much of it would be injurious." She could not be induced to give it up, because while taking it she had some peace of mind. In fact, it was the only way of peace for all concerned. She was not thought to be insane. Under its effect and that of stimulants she "could review and analyze, with calm indifference to the suffering, the mental operations of the past ten years." It came clear to her, she says, that her condition was one of insanity. She called it a case of "monomania," and regarded the ideas associated with C. as delusional. She had a comfortable feeling of independence in regard to her conscience, but if the influence of the medicine was not kept up, the mental pain came back. At the end of two years she had increased the medicine largely. Then she also called her father to account for a conversation she thought she overheard, which she found to be a hallucination, and ascribed it to the medicine. She readily recognized it to be such, and this is undoubtedly the only instance of the kind in her history. This, with the druggist's repeated warnings of harm, led to a strenuous effort to stop the narcotic. This made her angry, an unusual event with her; in a characteristic way she vowed she would take no more, and stopped at once. She was sent to the country with an intelligent companion, where she spent a month with great bodily improvement. She was perfectly correct in conduct and attracted much respectful attention. Soon after this, by her own desire, she was placed under special

treatment, and what followed has been narrated. In the first four months she gained physically, in weight and otherwise. Then came the affair of the laudanum and the diagnosis of insanity. Immediately before this she had written a long and interesting letter to her home. The physician's telegram to her relatives announcing her transfer to the asylum was followed by one from herself saying that she was well and telling them not to worry themselves.

The events of her first five months in the asylum have been described with sufficient detail, including the incident which led to a free revelation of her mental history. True to the characteristic habit of negation and antithesis, her pride and reticence had long been accompanied by a fear lest she should betray her secret, and then by an impulse to do so. Both the fear and the impulse grew upon her. But to detail her troubles to me soon came to be a great relief and comfort. Thus was developed another characteristic of these cases—the desire for never-ending repetition. Once engaged in conversation with me, there was no embarrassment in discussing the painful ideas—*no process of stopping for vows, etc.* There was but little material change in her mental operations for the year and a half while the analysis of them which has been given was going on. In general she was much more comfortable, she said, than before her long seclusion at home. This period of comfort represents a remission of the malady which is another of its characteristics; such remissions may last for months, or even years. She occupied her time very well in reading, sewing limited however to a few articles, etc. Later she was able to have some bits of ornament or color in her room. The morbid ideas continued oppres-

sive, but they were managed more methodically and readily than formerly. She was continually having distressing experiences, because of such circumstances as that the name of one nurse and the color of the eyes of another called up painful associations. She preferred that my wishes should be made known to her as directions rather than as requests, because she was thus saved from the difficulty always attending any voluntary action; but the aid gained in this way never amounted to much. Any act that was made a matter of my explicit command could only be obeyed through the system of vows; she could not follow the advice to act under a "new master," as against the imperative idea. The following description characterizes her mental habits fairly well. When she "came up against" any question of doing or not doing something, the thought of the act seemed instantly attended by a great crowd of associated ideas. There was, as it were, a first series of things of which she had to think in a certain order, because they were directly involved in some way with the contemplated act. Then the first series involved another series, and so on through a permutation of very diverse ideas, narrowing at last to the one idea, that through all this indirection the act would carry harm to C. Then, if strongly impelled to do the act, as for example if she wished it or needed to maintain appearances before others, etc., she had to "bind herself over" in the way heretofore described to protect C. from the possible harm. It sometimes happened that after she had bound herself to do or not to do some act, the contrary became imperative. Then there was a dreadful process of counter-vows and "binding over." The associated assemblage of ideas varied for different acts and from

time to time. When the ideas arose she had to run them all over in her mind in a recitative way, always to the same central and final idea. She became very expert at this, and speed was acquired by methodizing the process and by practice, so that she mentally touched the heads of things as she ran through the mental formula. Yet, strangely enough, she could carry herself with perfect serenity and self-possession in the eyes of all but her physicians. It required a great deal of study and analysis to disentangle the thread of evolution of all these subtle and complicated phenomena. She declared that she was then able to talk of them for the first time, but that she could give only the most meagre idea of it all. This writing itself is inadequate in representing what she did give. She said, "When I try to describe my years of trouble, so many distressing thoughts come into my mind that I cannot prune my words of what is not essential."

She always declared that she had no conscience, that no question of moral right or wrong influenced her; that ceased long ago. Her only criterion was to do as near her liking as was permitted by the tyranny of her controlling ideas. She said that in her desperation she did not hesitate in her thoughts to do anything, however wrong it might be, if she desired it, and she could accomplish it by her process of "binding herself over" under penalties for not doing it. In this way see could sometimes defy or rather evade the consequences of acts that at first seemed likely to be harmful. She said, if a duty came in conflict with the ruling ideas she yielded the duty, but if a selfish inclination came up she could evade the harmful consequences. She said also that she had long led two distinct lives: one was entirely within herself,

with its great complication of motives, ideas, and suggestions, and the other was a false pretense; she was a walking hypocrisy, possessed by an insane idea, without any rightness of conscience or natural affection, selfish and wicked and without repentance for many wrong and disgraceful acts intelligently committed. She betrayed her real conscientiousness by bewailing the wickedness of her conduct. She charged herself with misusing the system of vows which she thought should have been reserved for proper purposes of relief from her painful feelings, when trying to do *right* things; she misused a blessing, she said, in employing it to get indulgences for doing wrong. But she made too much of the questions of right and wrong in regard to ordinary matters. Her bewailment, however, was more a matter of intellection than of depth of feeling; the former was so forceful that it was accompanied by a large measure of the corresponding feelings which she knew she ought to have under such circumstances, were they real ones. The depression of feelings was from a sense of oppression of ideas. But through it all there was evidence that all of this was qualified by some consciousness of the fictitious character of that which dominated her mind. In other words, it was still true that, were the domination removed and normal ideation made possible with relation to its limited disorder, she would have been a well and happy woman.

In regard to abstract, painful ideas, not connected with any acts, the like need of her special system continued. Certain words always troubled her in her reading. During the comfortable period of a year and a half now being described she became able to read quite freely until the last, when it was almost impos-

sible. The word "murder" was the worst, and all others expressing acts of violence were almost as bad; next came such words as "jealousy," "hatred," "malice," "sin," and "shame." If, when reading, such a word appeared before her eyes, there was instantly in her consciousness the crowd of associated ideas, and along with them the thought of C. as the object of the act or the subject of the feeling indicated by the word, as if it were M.'s own act or feeling. Thus through some one of a multitude of chains of associated ideas the thought of harm to C. was reached from the "jealousy," "hatred" or "murder-thought." The effect of this had then to be annulled by the self-imprecation of some penalty upon herself if harm did happen to C. It appeared from this that the process of substitution of vicarious sufferers had been changed to one of self-imprecation after the time when she felt herself to be guilty of the deaths of the substitutes and had enacted the murder scenes. To avoid the pain that the future deaths of such persons might cause she chose the new method,—and it was as if she said, on all these occasions, "Let me be punished as guilty of all those acts and the new harmful thoughts, should anything happen to C." This substitution of herself protected C.; then, C. being protected, her own possible future remorse was warded off. When in the presence of others, this process might be dispatched quickly and unobserved; or, if reading, it might be necessary for her to put the book away and go to her room, and there, walking agitatedly to and fro, the whole process had to be gone over many times. On many occasions, at last, she had to find the book and the word repeatedly, and as many times begin anew. Again, the book must be taken to her room, and there

the reading of the word initiated a repetition of the process; she must seek the word and repeat the process many times. There were so many special words, and suggestions of painful ideas in innocent words, that reading became too painful to be attempted.

In the constant mental conflict that was going on the negations of conduct frequently involved questions as to what she ought to do and ought not to do. In this more comfortable period she once told me that a case like hers would be found in Mr. Howells's last book. She said, "Penelope Lapham had the same trouble I have, but not a millionth part as bad," and referred me to Penelope's struggle with her dilemma after Corey, contrary to expectation, had proposed to her instead of her sister, much to her own mental distress. Some time after this event Penelope said: "It's easy enough being sensible for other people. But when it comes to myself there I am! Especially, when I want to do what I oughtn't, so much that it seems as if doing what I didn't want to do, must be doing what I ought." This saying, and others like it, were so true to nature as to make Penelope's case distinctly one of the "insanity of doubting" in one of its more common and milder forms; but fortunately she "recovered," and this was by the logical process of the substitution of the more dominant idea strengthened by the deeper feeling.

During the latter part of this period she was allowed to go about alone in the neighborhood of the asylum "on parole." No explicit promise was exacted, however; she was directed not to go beyond certain limits, with a tacit obligation on her part not to do so. She was better for this, and the privilege was continued for a number of months after she entered upon the next period, which is now to be described.

She afterward said of herself that a crisis seemed to come in her mental condition. All the complications and agitations reached such a tangle of difficulties that she felt powerless to cope with them and seemed to come to a standstill. She said that her conscience reasserted itself in a natural way and began to punish her for her wickedness by compelling her to do everything contrary to her wishes. There was now a real depth of feeling, though morbid, that she deserved punishment, and her conscience seized upon every opportunity for inflicting mental pain; this she felt to be simple justice. At this point in the case there is to be recognized an exacerbation common in the course of such maladies; but more than this, there was now initiated a state that more nearly resembled true melancholia—an essential disorder of the feelings. The gloom and despondency became more pronounced. She had to make herself disagreeable and hateful to the people she liked the best and respected the most, just because they were sources of comfort to her. She said: "It is as if all the wrong things I have ever desperately allowed myself to do and think about now stand around me as creditors of my conscience." A denial of everything pleasant or desirable was thus commanded. She had to speak unkindly to her favorite nurse, for example, and then cried because she had been compelled to do what would make her appear ungrateful.

A change now came about in regard to her relations to me. At first, as has been said, her revelations to me were satisfying, and she had little of the temptation to tell her story to others, which she formerly feared she might be compelled to do. Her sole source of comfort was in such conversations, but the need of

this grew upon her, and at last it became impossible to give her time enough to satisfy her. The temptation to tell others returned with the worry about it, and in the end she yielded to it in some degree. It also came about that she felt the comfort she gained from my visits should be forfeited as one of her punishments. This became the prime issue in her struggle with her avenging conscience. She fought it for months, avoiding meetings with me, "vowing" she would never tell me another word of her troubles, but just as often breaking her vows and talking freely whenever in my presence. Her sufferings increased because these things involved harm to C., and new penalties, and only grew from bad to worse. She began to say, "I must go from here to avoid this suffering; there is no other way. The only hope I ever had has been here, but this I must abandon; anything is better than the suffering I endure here. I must be deprived of my greatest blessing." She felt that if she went away she must leave hope behind. And so the culmination of this phase of the mental phenomena was gradually reached.

All this state of things was believed to be a passing phase of her malady, and she was urged to hope that another remission would come. She could possess herself of this while it was being told to her. A curious thing came about at this time. Not only was the comfort of hopeful words intensified to her, but it was reinforced by telling her how foolish and absurd her mental performances were. But, true to the habit of growth of her morbid mentation, there was need of greater emphasis in the telling to give her equal comfort. At this time an interview usually ended by her saying, "Now, before you go, you know what I

want you to tell me—tell me what you think of me. It helps me to hold on to the idea; what you say seems all real and true to me, but it goes when you go, and I am as helpless as before." As the effect of this, the kinship of human minds had an illustration. My gentler characterizations became chidings and rebukes, and at last denunciations, which exhausted my polite vocabulary. It was curious to observe the calm content with which these objurgations were received, and a little startling to realize the personal comfort of such an unwonted outlet for sentiments that must be habitually repressed. Thus it came to pass that this poor patient brought it upon herself to be the "vicarious object" of stinging words which might have been more fitly spoken.

But now there came a climax in this pitiful history. Like a law of mental habit, the idea of vicarious atonement or substitution that had run all through all these phenomena from the first came into play again. What was told her made her seek for hope though she could not have it. It occurred to her that she might substitute physical for mental suffering—some serious illness or great calamity—some personal injury that would stand in her mind as a lasting token of ample punishment; then the demand for penalties would be satisfied, and her mind would be left free to enjoy in peace the common comforts of life. She invented a plan which bore some resemblance to her former suicidal scheme. On taking a morning walk outside the asylum, she procured a small pistol, and late in the evening, when her neighbor, whom she did not wish to disturb, was out of her room, she shot herself in two places, the shoulder and the hip, making simply flesh wounds. She was perfectly calm about

it and explained that she tried to wound the joints, which she thought would cause permanently painful and crippling injuries. She protested that she carefully avoided endangering her life; for she had virtually pledged herself not to do that. The wounds were trifling and soon healed, her disappointment knew no bounds; the last hope was gone, as she felt; her mental depression rapidly deepened in a kind and degree unknown before; in death was her only relief; she regretted the lost opportunity when she had the pistol. There was now a condition like true melancholia; the suicidal impulse was pronounced, and honestly declared.

She was of course closely watched. Her former state continued and grew worse; she punished herself in every way—in regard to taking food, and in personal care, etc. She would stand in a fixed attitude unable to move for a long time. She contrived new ways of mental punishment—for example, another patient thought she had committed the unpardonable sin, and M. assuming this to be the worst of all sins, imprecated upon herself the punishment, whatever it might be, due to that sin. This idea, however, was so intangible to her that it failed to become embodied in her system of thought. Even in her state of true melancholia there was yet something lacking of the profound feelings of depression and self-condemnation characteristic of that state.

On a later occasion several attempts were made to hypnotize her—to try the effect of "therapeutic suggestion." She did her best to aid in this, but was more amused than otherwise by her swift conclusion of its futility. She said it never could be done, the prime obstacle being that she could ordinarily never go to sleep

without having first cleared, or exorcised, from her mind all the thoughts of evil consequences which had attached themselves to the events of the day and to which she felt that she might perhaps not have done full justice. The poor woman's prayers even had been for years imprecatory. She had, at times, piously prayed for help and strength, but the thought of possible consolation from this source made such relief a subject of punitive deprivation; moreover, her dreadful misuse of the privilege of addressing the Deity debarred her from the proper use of it. Another of her peculiar methods was in some respects not unlike the last. There were times when she could gain herself a respite for a day by first binding herself over to do everything in the natural way for twenty-four hours and not to make any "vows." She occasionally got a great deal of comfort from this device, but at the end of the period she always had to review it and balance accounts in detail. At last, however, after her conscience "had turned upon her," there was an end to this expedient.

This history, already a long one, must be ended here. In this, M.'s last and worst state, it finally seemed best to try the effect of some change, which she earnestly desired, and she was transferred to another asylum. She bore herself passively, but with an evident feeling of regret and as if she had indeed left hope behind. There was immediate improvement in manner, and in the course of a few months her condition became much more comfortable. The change to new surroundings to which she felt indifferent was a great relief. Freed from the oppression of the questionings as to talking with me, she remembered helpfully my explanations, reasonings, and advice. A year

later she was as well as I had ever seen her, appearing very well, intelligent, and ladylike to all who knew her. The secret mental processes went on, however, and, though more submissive, she had little hope. She said her coming to meet me was opposed in her thoughts as formerly, but in her strong desire for the interview there was no hesitation in determining to have it in spite of all consequences. There were no preliminary vows, but she knew she would "have to pay for it afterward." It was natural, vivacious, and most interesting in the abundant proof it gave of mental integrity aside from the limited derangement. At its close she begged me, in the old way, to assure her that it was right to have seen me. No definite information as to her subsequent history has come to my knowledge.

The details of this long story were corroborated in many ways by the agreement of their many repetitions when discussed from different points of approach. Like most persons under the control of morbid ideation, she was in the habit of saying to me, "You would have a different opinion of these things if you could only know more about them; they are so many and so complicated that they cannot be told." Every new and often repeated analysis of the essential nature of her mental processes was readily understood and gladly accepted by her, only to be followed by some new phase of ideation, or memory of past trouble, which would stand more positively in her consciousness than the clear explanation of it all, which she constantly tried and failed to apply.

This history may be briefly summed up. The telling of the story is meant to show the natural and logical growth of complicated phenomena from the central morbid idea; they depended upon this as the limbs,

twigs and leaves hang upon a tree. However distinct and strange any newly revealed product of her system of thought might seem to be, its unity with the rest clearly appeared upon analysis. The minor formulae of her mental operations were all more or less insistent or imperative, strictly according to the closeness of association of the ideas with the central and ruling one; and corresponding also to the degree of fixation of such methods of mentation, by repetition, practice and habit. All these products of mental phenomena are explainable upon the basis of normal psychical law.

The problem of this case was to discover the genesis, the growth and the fixation of the central idea, which in this instance had the peculiarity of being unusually complicated. In the first place, there was probably no special hereditary influence in its origin; the paranoiac element is excluded; certainly the right to infer it must be questioned. If it be said to have been "acquired" because of the typhoid fever at the pubertic period, etc., a neurasthenia must be admitted. But if acquired organic defect be admitted also—while inquiry is excited as to the consistency of this inference—it remains that the "fixed idea" was conceived some years before, disappeared, and was revived and developed when the supposable new factor of paranoiac defect came in.[1] But not only was the conservative

[1] There seems to be confusion and inconsistency in the meaning of terms used in the discussions of this subject. The terms psychosis and degenerative psychosis admirably characterize certain general notions of the mental states they represent, but as definitions there is danger of their use as cloaks for conjecture. Most writers hold that fixed ideas "*almost always*" have a neurasthenic basis; this rates them as psychoses and functional. But as ideational psychoses they lie in the foreground of primary systematized delusions typical of the admitted degeneracies in paranoia. Hence the temptation to unify the view and to regard *all* the "imperative conceptions" as degenerative psychoses; under cover of this term they are brought

tendency to maintain intellectual integrity significant; so also was the absence of the characteristic habit of symbolism. Paranoiacs are prone to symbolism, whereby meanings are betokened by trivial things, and delusional ideas are quickly conceived—for example, the accidental placing of three apples in a row might be believed to represent the Holy Trinity. For this reason there was a negative importance in the limitation of the strong disposition to substitute persons and things the one for another. This did not bring out any innate tendency to symbolism, as was likely to have happened had it existed. There was

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into the category of paranoia. But this term distinctly introduces among the psychoses the new notion of chronic hereditary or acquired defect in the organism. Upon a foundation of heredity it is easy to conceive such organic defect; to invoke a like foundation as "acquired" through a simple concurring neurasthenia has the vagueness of assumption and conjecture. These, therefore, have really no support, in the case of fixed ideas, except the fact of the ideational origin of the latter and their kinship to paranoiac delusions. But the clinical fact remains that their milder forms, at least, are commonly incident to healthy minds; often independent of neurasthenia, they make no trouble, and fail of recognition because they are so little out of harmony with concurrent conditions. Starting with these mild forms, may they not be purely functional, even along with neurasthenia, for some degrees in the scale towards their culmination in the admitted degeneracies? The recognition of two general degrees of fixed ideas (answering to psychoses and degenerative psychoses, between which differentiation is as impossible as to mark the changes from youth to old age) is not out of analogy with the same division that is claimed as necessary in describing delusions. Cf. pp. 227–8.

The *Journal of Mental Science* for January, 1888, received just as this article is going to press, contains the conclusions, p. 532, of Professor Kovalewsky in an interesting article on *Folie du Doute*. He recognizes several degrees, and conditions of origin, of these affections. But he starts with neurasthenia as the common soil from which they spring, belonging to the group of degenerative psychoses including pathophobia and the other forms of these ideational disorders; and neurasthenia itself is regarded as rarely acquired or other than purely hereditary in relation to these cases. This fails to recognize the larger background in the normal field of intellection, where the accidental and irregular coördinations of idea and feeling, indulged, or otherwise fixed by habit, are the germs and often growths which are not degenerative but spring up more readily in soils of purely acquired neurasthenia, or of hereditary degeneracies.

always validity in the factors of her mental equations; the thing substituted was equivalent to the other in quality or effect, and there was logic in the process.

At the root of the whole matter there was a bodily condition somewhat neurasthenic. There was also an unusual intellectual endowment and rare conscientiousness. Upon the mental plasticity of childhood strong impressions were made by unpleasant ideas about "trance" and the horror of being "buried alive." Apprehensions for herself and relatives were engendered in this regard. Here was a quick soil for the natural questionings as to the right and wrong of her conduct to grow to be questionings as to whether her acts were right in respect to their effect upon herself or those dear to her. It was but a shade of change to conceive that her acts were not right in the sense of not being safe, which includes the idea of harm as a consequence. This is the usual significance of the phrase "not right" in these cases. The attendant fear of harm was natural and logical. This combination of the idea and its attendant feeling once formulated, the "impression" once made, the idea became "insistent" and then "fixed," and the "path" was formed by repetition and habit. Here then was the "fixed idea" that some of her acts were not right, which included the idea of harm. This is a common and primitive form of fixed ideas; it was the root of all that grew up afterwards. Next followed the necessity, common in such cases, of repeating acts to make them right, which led to the doubting and hesitating over an act as to whether it was right or not, and to the repeating, doing and undoing of acts. Thus is explained the paradoxical "doubting" associated with

"fixed ideas." Here next was the basis of the habitual negations of thought and conduct—the doing offset by undoing, the balancing or cancelling of one thought or act by another thought or act—the substitution of one thing for another. From the root-idea that an act was not right came its larger growth and its offshoots, intensifying the impression of the primary idea upon the organism. This was further intensified by the feeling of fear normally attending the idea of harm, the impression of the whole being deepened by habit; its hold upon the soil being strengthened by its growth.

At the beginning, before the age of twelve, the seed of the malady was planted. Its growth was arrested and she appeared to be well till three years later. It might not have appeared again had it not been favored by physical debility. A new growth began; she repeated her acts to make them right. She was relatively backward in her studies, and had a natural desire to excel her companions, which she did. Her love of admiration and attention was perfectly natural, as was her feeling of rivalry towards C., whose unusual loveliness of character and person was more attractive than her own superiority of intellect. Such was Madame de Staël's life-long envy of beautiful women. Such thoughts and feelings are common enough among the young; it was natural enough that her sensitive conscience should reproach her. Thus, with the mental habit already formed there was set up a self-inquisition as to her feeling toward her friend, whom she was afraid of hating, as she soon morbidly accused herself of actually doing. It was but a step then to the thought of harmful things that might happen to her friend, and but another to self-

condemnation for thinking of them, as making her equally guilty as if wishing them to happen. Of course there was exaggeration of feeling in this, but so far in its development there was not, necessarily, anything that might not be common to many conscientious young people. This idea of harm attending her acts, and the accompanying feeling of fear, readily became a concrete idea of harm to C., and to this was added the conception of her remorse as a consequence of harm to C., as if she had wished it. This of course strengthened her specific fear of harm to C., and reinforced the general idea of harm, and therefore the impulse to repeat her acts. Ultimately the question of harm to C. attended all her acts. This established the central idea and mental habit out of which came all its after-growth. From this point the evolution of the mental phenomena was plain enough, as given in the details of its history. The need of repeating acts and "vows" many times—often a definite number—for the sake of emphasis, was a manifestation which characterizes what is called the "counting" variety of cases of fixed ideas. This did not become a prominent feature of this case because it was left behind by larger complications. The substitution process was most curious and constant, but it was only an outgrowth of the primary fact in these cases of doubting, balancing and offsetting one thing against another in an endless series of negations. This process was also one of the remarkable devices for circumventing the dominating idea, as if the characteristic paralysis of the will was limited in respect to that, and the fixed idea stood as an irresistible intruder in her consciousness, while otherwise the will and the intellect were free to evade its control. The murder-acts were simply

dramatic as far as homicidal impulse was concerned. They were extreme examples of what is common in such cases, particularly in the "metaphysical" variety,—an idea at first repulsive worries by its insistence, becomes involved in a process of negations, and at last reasons itself into dominance. Such transitions from one extreme to another of thought and feeling are but exaggerated examples of the law whose order we follow when

"We first endure, then pity, then embrace."

The kindred nature of the great variety of these affections is well indicated by the broad designation of "insistent ideas," as was suggested in the introduction to this article. These aberrations from the normal of well-balanced ideation and feeling, being once initiated, may develop graver forms in cases of the more positively degenerative type, because of paranoiac heredity or acquired defect,— in other words, when there is hereditary or acquired nervous and mental instability; and such cases may more or less quickly develop primary delusional insanity. The commonness of these affections in their milder forms as simply insistent ideas, and their outgrowth from the ordinary and natural operations of the mind, is a matter of great interest. The commonest superstitions, and idiosyncrasies of formulations of ideas and feelings, which control conduct and enforce habitudes, are of this order in their slight departures from sound reasoning. There may be many degrees of these affections before they are recognized as positive disorders. This view of their common origin can best be illustrated by a series of cases in which the early phenomena are more nearly within the range of observation. But even the history of this case is not

inconsistent with this view; nor is it so, as against that of their origin being usually constitutional. For the same reasons it is hardly necessary to search the mysteries of "the inconscient" for the genesis of phenomena which admit a simpler explanation. Many of these affections are to be easily understood as simply accidents or idiosyncrasies of ideation, arising from an incongruous association of ideas, happening according to the common law of contiguity, and becoming fixed in proportion to the intensity of the impressions and by the laws of habit. This may happen in a healthy brain, by mental shock, or by a slower process. This case is an example of the operation of these laws in a plastic organism, with the qualities of a sensitive conscientiousness and fertility of thought, favoring the particular idiosyncrasy. The morbid premise of the fixed idea being once established, it had its own logical sequences. The collateral ideation was normal enough and continued so for many years. There was normal sensibility and feeling except in relation to the fixed ideas and their complications. The will was dominated only in respect to these, and otherwise free and efficient.

The origin of these affections is distinctly ideational, but they well illustrate the inseparable nature of thought and feeling, especially when their genesis is favored by disordered states of feeling. The laws of habit play a most important part in the fixation of such ideas, and much is here to be learned of the nature of the great influence of habit in all forms of insanity. These affections, through the possibility of their comparative isolation, permit the study of the formation of delusions. Again, the rôle of the attention is a leading one in these mental phenomena; the attention is com-

manded in proportion to the insistence of the ideas. In extreme cases of this kind of limitation of the attention there is, side by side with it, more or less activity of consciousness. But it is in this direction that these mental states merge with those described as characteristic of the hypnotic state in its varying degrees of unconsciousness, and of the "limited attention" peculiar to that state. That condition characterized by Professor Stanley Hall's phrase, "tonic cramp of the attention," is most strikingly shown in these affections. The law of suggestion of ideas is active, and even by auto-suggestion the mental attitudes are induced so analogous to physical "cramp." Also the relation of these attitudes to physical reactions is striking. "A diffusive action in the nervous system accompanies all emotion"; for example, a common sequence is fixed idea, fear, pallor, and heart disturbance, etc. It seems in many cases as if the "path" between the ideation and the sympathetic nerve became so open and direct that there is the changed sequence of fixed idea, heart-quake, and last the conscious fear. At all events this last is swiftly overtaken by the automatic organic attitude of fear; and this quick reflex from the idea undoubtedly increases the fear. The man is frightened by his own trembling; he is "a coward upon instinct." This amounts to mental suggestion from the physical field, just as when the hypnotized are put into bodily attitudes that suggest hallucinations. Again, the alliance of these mental states with some forms of "hysteria" is undoubted, and explanatory of it. The medico-legal importance of these conditions occupies a wide field; this very case presents interesting features in this regard.

All these things show the unity of the great problem into which the clinical study of mental phenomena leads. The propositions here advanced involve such important questions, and more than these, in the investigation of this subject; and this case is interesting as presenting so much of the evidence that has led to these views.

A CRITIQUE OF PSYCHO-PHYSIC METHODS.

BY JOSEPH JASTROW, PH. D.

Probably no result of experimental psychology has been cited as frequently and with as much confidence in its truth and importance as the psycho-physic law. By some it has been regarded as worthy of ranking with the law of gravitation, while others refuse to recognize it at all. A clear statement of what the law means is rarely found; sometimes the term refers to the results of Weber's experiments, and again it refers to Fechner's deductions concerning the relation of stimulus to sensation. But these two are in a sense totally distinct, and should be kept so. The psycho-physic methods are applicable only to such experiments as can be utilized for establishing Weber's law. And this paper is to be devoted to a rigorous logical criticism of the methods and interpretation of such psycho-physic experiments. Its object is a practical one: to clear the way for a more rational system of psycho-physics by directing future experimentation into that path in which it is most promising of results, and thus preventing the employment of the many uncritical and unanalyzed processes now current.

Where the falsity of one point is so closely connected with the falsity of many another, it is difficult to know where to begin and how to proceed. The full appreciation of one point requires a knowledge of the con-

siderations that follow; hence the order of exposition will have to follow convenience rather than logical sequence.

The Three Psycho-physic Methods.

And first let us state briefly what the three usually recognized psycho-physic methods are:[1]

I. *The Method of the Just Observable Difference.*—This is the method that Weber followed in his celebrated experiments, and has, I believe, done much to introduce radical misconceptions into psycho-physics. This method consists in applying a certain stimulus to the sensitive surface of the subject, and then finding the least greater or the least smaller stimulus which can just be recognized as different; one either adds small increments to the initial stimulus until the stimulus thus formed is felt to be greater, or lessens the intensity of the initial stimulus until the diminution is clearly noticed, and notes in either case the ratio of the alteration to the original stimulus. It is considered best to employ both these processes, and regard the mean of the two results thus obtained as the true just observable difference. The ratio of the difference between the initial and the altered stimulus to the initial stimulus, or better, to half the sum of the two, measures the differential sensibility. The smaller this ratio the finer the sensibility is said to be.

II. *The Method of Right and Wrong Cases.*—One here chooses two slightly different stimuli and presents them to the sensitive surface of the subject, requiring him to judge whether the first stimulus is

[1] Other proposed psycho-physic methods, and especially that of the "mean gradations" (*mittlere Abstufungen*), deserve a separate treatment; this I hope to furnish on another occasion.

greater or less than the second, and records the number of cases in which the decision is right and the number in which it is wrong. The ratio of right answers to the total number of answers measures the sensibility and varies in a direct sense with it.[1]

III. *The Method of the Average Error* (or as I prefer to call it, of the Probable Error).—In this method a stimulus is presented, and the subject is required to adjust a second stimulus so as to be equal to the first. The average deviation of the several adjustments from their mean (or better, the probable error of the adjustments) directly measures the sensibility.

Critique of Method I.

Applying the method of the just observable difference to pressure sensations, Weber was led to formulate his well known law. In this he announced that if you apply a certain weight to the skin and find the least greater (or least smaller) weight which can be recognized as different, and then take an entirely different weight and repeat the process, the ratio of the first to the second of each pair of weights used in any such experiment will be the same; that is, the ratio of the just observable difference to the initial weight is a constant. This process simply compares the recognition of a difference in one part of the psychic scale with that in another; it says nothing and cannot be made to say anything about the ratio of stimulus and sensation. It is not a psycho-physic, but a psycho-pyschic, law. So much is plain.

But the experiment of Weber is in every way vague and inexact. To begin with, the bare statement that

[1] This is the usual form of the method. Variations in this proceeding will appear later.

one pressure is "just distinguishable" from another is altogether indefinite. This expression may have as many as four practically distinct meanings. It may refer (1) to two pressures sufficiently different to enable the subject *sometimes* to tell which is which; or (2) to differences that will *always* be correctly recognized; or (3) such as will only accidentally fail to be recognized; or finally (4) it may refer to differences which will be correctly judged any characteristic proportion of times between these extremes. Now the just observable difference will evidently be a totally different thing according to which of these interpretations is chosen. The first interpretation has rarely if ever been used, because in practice any difference that is likely to occur, however minute, will be sometimes correctly appreciated, and will sometimes arouse a confidence in the recognition of it sufficient to hazard an answer upon; yet it has quite as much in its favor as any other interpretation, and leads to equally definite conceptions. The second is the method usually chosen, but is almost sure to degenerate into the third. Under the fourth head any difference of stimuli correctly appreciated any given ratio of times may be used; the simplest is that in which in half the trials the difference is correctly judged, and this interpretation has found favor with some workers.

A very brief consideration of the psychological processes involved in such judgments is sufficient to show that in practice none of the four interpretations is useful or valid. The answer which the subject gives may be of two kinds: (*a*) if he is asked to mention *when* he feels disposed to regard the sensations caused by the two stimuli as different (and probably, too, noting the direction of this difference), his answer depends mainly, if

not entirely, on the minimum amount of confidence upon which he finds himself disposed to make a judgment; (*b*) if he is asked to decide which he judges to be the more and which the less intense stimulus—in other words, if the correctness of the answer (made on the basis of a certain stimulus) is the deciding point and not the disposition to answer, then we are simply using the method of the right and wrong cases in a rather loose form. As a matter of fact, the just observable difference method pure and simple has seldom if ever been used; the correctness of the answer is always to some extent taken into account and in this form the method loses all *raison d'être.*

Again, if we take Fechner's definition of the just observable difference as that difference which will always be correctly appreciated but which cannot be lessened without forfeiting this distinction, we are simply making a special method of that instance of the method of right and wrong cases where (in a limited number of trials) the number of right answers equals the total number of answers. As already noticed, an error here and there is usually considered allowable; but if a few, why not more? Is there anything but the arbitrary preference of the experimenter upon which to base a decision? A more radical objection, however, remains to be noticed. Theoretically a difference of stimuli great enough to ensure a total avoidance of error must be infinite. With a certain ratio of stimuli one may as a matter of fact find no errors, but there is no (theoretical) assurance that if the experiments were sufficiently continued an error would not occur. This point is too obvious to need further illustration. We have seen that in its pure form the method of the just observable difference

simply measures the confidence, the disposition towards answering, and only with whatever accuracy the confidence can be experimentally proved to be a measure of the sensibility, has it value in determining the latter. In the form in which the method is generally used it is simply a loose and inaccurate application of the method of right and wrong cases. The method may be of service in determining roughly within what limits it is advisable to experiment, and has other obvious (practical) uses.

If the mischief to which this spurious method has given rise were confined to the charges already brought against it, the case, though serious enough, would not be as serious as it really is. In addition the method has given rise to radically wrong conceptions, chief amongst which is the conception of the threshold (*Unterschiedsschwelle*). This conception grew directly out of the method of the just observable difference; in fact this difference has by some been taken to be the differential threshold. What is more usually denoted by the threshold is the smallest difference that can be perceived. It is the threshold of consciousness. The moment we define this term accurately its unscientific character becomes apparent. The threshold is described as a point not exactly constant, but nearly so; above it all differences can be felt, below it all differences vanish into the unconscious. No matter whether little or much below this point, they are all utterly lost; it is idle to say, as Fechner at times does, that they differ in the amount of additional stimulation necessary to bring them up into consciousness, unless you mean that the series below the so-called threshold is an exact continuation of the series above it—and if you do mean this, then the threshold loses all its distin-

guishing peculiarities and ceases to exist. Either there is a threshold—be it a point or a more or less variable line—below which is homogeneous unconsciousness; or from the region in which the *sensed* difference has its maximum of clearness down to the point where it utterly vanishes because the difference between the stimuli vanishes, there is a continuous series of intermediate degrees of clearness, and there is no point on the curve with characteristics peculiar to itself, no threshold in any true sense.

But it will be well to postpone further consideration of this point until the nature of the method of right and wrong cases has been delineated, merely calling attention to the fact that, as Fechner and others admitted or even vaunted, the method of the just observable difference is the only one that is closely or at all connected with the threshold theory, and naturally the two may be discarded together. Sensation and stimulation each forms a continuum, and it leads to hopeless confusion to apply discrete conceptions to them.

Critique of Method II.

The method of right and wrong cases is a device by which the sensibility can be determined while the judgment has but the simple problem of greater or less, of yes or no, to deal with. The factors of which this method makes use are the two stimuli of which the larger bears to the smaller a certain ratio to be known as $1+x$; and the ratio of *wrong* answers to the total number of answers to be known as n. While the ratio $1+x$ may have any value whatsoever, it actually does not differ much from unity, because only by the employment of such ratios can a number of right and wrong cases be readily collected. The all-important law,

justified by theory as well as by practice, announces that *ceteris paribus* as you diminish the difference between the two stimuli the number of wrong answers will increase, and as you increase the difference between the stimuli the number of wrong answers will diminish. In other words, as $1+x$ increases n diminishes, and as $1+x$ diminishes n increases. This law, that the number of wrong answers and the difference between the stimuli vary in opposite senses (the nature of this variation is not now in question), I regard as *a* if not *the* fundamental proposition of psycho-physics. (There are some conditions as to the nature of the experiments which must be complied with before the law will be found good; these will be assumed for the present and discussed later on.)

The all-important point is to decide how this "inverse law" is to be interpreted. In the first place, having found it true when the two stimuli differ by a quantity x, we expect to find (and will find) it true when they differ by $\frac{1}{2}x$. As, however, the judgments of the subject are under the influence of the many slight variations of condition that always influence psychological processes, it is possible that in a particular instance (especially when x or the total number of trials is small) the difference in the number of wrong answers will fail to appear. But if the number of experiments be increased, *and be increased the more the smaller the value of* x, this difference *will* appear. It must be remembered, however, that it may be impracticable to collect sufficient observations to bring out the more minute differences. But we have a right to infer that under proper conditions they would appear. If I find that as I successively experiment with stimuli that are related as 1 to $1+x$ and then

with stimuli related as 1 to $1+\frac{3}{4}x$, and then as 1 to $1+\frac{1}{2}x$ and as 1 to $1+\frac{1}{4}x$, the ratio of wrong answers, n, successively increases from one to the other, it certainly is in the highest degree improbable that the law does not hold with intermediate fractions of x. In other words, we infer that it can be expressed by a continuous curve, and that we must theoretically regard the probable ratio of wrong answers with two stimuli differing by the ratio x as smaller than the ratio of wrong answers with two stimuli differing by any fraction of the ratio x (1) in general, no matter what the value of x is, and (2) in particular, no matter how near zero that fraction of x is. Any one admitting these propositions (and it is not clear on what grounds they can be questioned) must logically endorse the psycho-physic reform which I am about to advocate; and I cannot but think that if psycho-physics had been built with a due consideration of these propositions, that science would have been a different and a sounder one. As a matter of fact all, even those least in sympathy with the point of view here taken, admit the law (1) in general when the value of x is confined within certain limits, and (2) in particular when the value of x does not too closely approach zero. Of course they have not stated their position in this obviously incorrect way; but if asked to state it they could not, as I understand it, state it in any other way. To show how totally without justification such a position is, it is sufficient to state that the choice of what values of x shall be admitted and what excluded, as well as of what limit is to be set to the lowest value of x, is and must be to a large extent an arbitrary one.

We can now return to the discussion of the theory of the threshold. What from the point of view of the

method of right and wrong cases does the current conception of a threshold demand? Nothing less than the position just now refuted. It has been proved that the ratio of wrong answers increases as the difference between the stimuli decreases; but the "threshold theory" claims that this law fails to hold after this difference has been diminished below a certain ratio. It actually says that you will oftener err in judging between weights of 30 and 32 ounces than in judging between weights of 30 and 34 ounces; oftener in judging between 30 and 31 ounces than between 30 and 32 ounces; but (supposing the so-called threshold to be $\frac{1}{30}$) that you will NOT err oftener in judging between 30 and 30.5 ounces, or between 30 and 30.1 ounces, than in judging between 30 and 31 ounces. Or, from a psychological point of view, it must propound the strange proposition that while under favorable conditions you will be enabled to appreciate the difference between 30 and 31 ounces, the conditions will never be sufficiently favorable to enable you to appreciate the difference between 30 and 30.5 ounces. Although the conception of the threshold is made highly improbable, and even irrational, by the consequences to which it inevitably leads, it is not necessary to be satisfied with a theoretical refutation. One can experiment with several differences all well below the limit assigned as the differential threshold, and actually find out whether or not the ratio of errors with the smaller of any two of these "sub-minimal" differences will be greater than that with the larger. Mr. C. S. Peirce and the writer undertook such a series of experiments (*v. Memoirs of the National Academy*, Vol. III), and found most conclusively that the law that the ratio of errors varies in an inverse sense with

the difference of the weights to be distinguished holds good as far as it is practicable to test it, and presumptively holds good throughout. It certainly holds good far below the limit assigned as the differential threshold for pressure, and if experiments could be sufficiently accumulated would be found good for still smaller differences. I do not forget that it would require an almost infinite series of experiments to make the most minute differences appear; just as at the other end of the scale one would "never" err in judging between 1 ounce and 12 ounces, and "never" in judging between 1 ounce and 13 ounces; yet it leads to more correct and practically useful conceptions to assert that an error is more probable in the former case than in the latter. This train of argument will be perfectly familiar to mathematically minded persons. (For a further discussion of the threshold *v.* Appendix A.)

We are now prepared to consider with more thoroughness the nature of the method of right and wrong cases. While, as the name of the method implies, the experiment is to be so arranged that the subject is to have the choice of two, and no more than two, answers, one of which shall be right and the other wrong, only a small number of experimenters have followed this rule. The violations of it have been of two kinds: first, in allowing the subject *three* answers (by using three kinds of pairs of stimuli, viz., having the first stimulus greater than the second; having it equal, or having it less); second, in allowing the subject the privilege of answering in one of these ways, or of saying that he is "doubtful." The objections to the first of these proceedings are evident and conclusive. As will be presently seen, one of the conditions on which the validity of the method of right and wrong cases depends is that the number of answers correct by the

action of chance shall be known; and the possibility of three answers introduces an awkward and useless confusion into the calculation of these chances, and requires a much larger number of experiments to ensure equally reliable results. For in each case the answer given by the subject may be (1) correct, (2) singly wrong, and (3) doubly wrong; *e. g.* if the first stimulus is really more intense than the second the subject may call it greater and his answer be correct, may call it equal and his answer be singly wrong, or may call it less and his answer be doubly wrong. The calculation of the chances of a correct, a singly wrong, and a doubly wrong answer by mere guesswork is certainly a very delicate one, especially when you consider that when the stimuli are really equal a doubly wrong answer is impossible and there are two ways of having a singly wrong answer. Again, with fine differences it will be difficult to keep the three kinds of sensations in mind, and slight lapses of the attention in such cases would favor the judgment that the stimuli are really equal. But objections to this proceeding could be indefinitely multiplied,[1] and one very curious one is given further on. Suffice it to say that it has no point whatever in its favor, and is really antagonistic to the spirit of the method upon which it foists itself. The method of right and wrong cases is a justifiable and a good one for measuring sensibility; the method "of right, wrong, or equal cases" is certainly a different one and has no justification.

The objection to allowing doubtful answers is also apparent. If you do allow them, what are you to do with

[1] A very serious objection is that it will be difficult, if not impossible, to take adequate account of the difference in sensibility for an increase and a decrease of sensation, which, it will be shown later, it is necessary to do.

them? Neglect them? Then when your x is very small you will have to throw out most of your experiments, and will in fact be recording only the best ones; while the very condition that makes the method of right and wrong cases a valid one is that *all* errors be recorded. Errors are due to slight lapses of the attention and all the other fluctuations to which the judgment is subject; to allow doubtful answers is to rule out all cases in which the judgment is in a somewhat worse than its average condition, and thus to vitiate the real average. It would be quite as justifiable (in fact, if you do the one you ought to do the other) to rule out all cases in which the subject feels *unusually certain* of the correctness of his answer. But may we not, as Fechner[1] and many others did, count half of the doubtful answers right and half wrong? Certainly not. (1) Because all judgments must be recorded as given; (2) because that would give a fictitious appearance of having made more observations than you really have, and when x is small (and the number of doubtful answers large) would seriously influence the meaning of the result; (3) because, while it is true that the chance of any answer in general being correct is one half, it is not at all likely that the chance of this particular kind of an answer being correct is as much as one half. Other more practical objections to the process are that it encourages fatigue and diminishes the regularity and simplicity of the judging process. In fact the only point in favor of this proceeding is that this doubtfulness is a real and valuable symptom; but a truer and

[1] Fechner used this method at first, but gave it up later. It is to be noted that if you allow "equal" answers you *certainly and especially* cannot count the doubtful answers half wrong and half right, because in that case the chance of a right or wrong answer is not one half.

better mode of taking this into account is described under the term "confidence" in Appendix B.

We are now able to define the method of right and wrong cases, and having done so, may pass on to its theoretical justification. The method may be formulated thus: Having chosen two stimuli of which the one bears to the other the ratio[1] $(1+x)$, apply them to the sensitive surface of the subject, requiring him to decide in each instance whether the first stimulus is greater or less than the second (one of these answers being right and the other wrong, so that by mere guesswork he will answer correctly in one half the cases), and record the ratio (n) of wrong answers to the total number of answers.

As already stated, the errors may be regarded as due to lapses of the attention, slight fatigues, and all the other numerous psychological fluctuations that go to make us now better and now worse judging agencies than our average selves. These influences may be said to have the effect of loading the smaller stimulus (or lightening the larger) so as to come up to (and, strictly speaking, just overtop) the greater stimulus. As long as $(1+x)$ remains constant, the "amount of work" which these accidental fluctuations must perform in thus loading the smaller stimulus sufficiently to cause an error remains constant, and is really x; hence, as $1+x$ diminishes this work diminishes, and as $(1+x)$ increases it increases. These fluctuations, it is understood, are of such a nature as to be as frequently and to the same degree in favor of our judging powers as antagonistic to them; and the probability of their accumulating sufficiently in one direction to cause an

[1] $(1+x)$ and not x is chosen to show that the ratio of the larger to the smaller stimulus is designated; this choice is made on grounds of convenience only.

error when the first stimulus bears to the second the ratio $(1+x)$ is less than the probability of their doing so when the ratio of the two stimuli is less than $(1+x)$, and is greater than when that ratio is greater than $(1+x)$—*i. e.*, with a smaller $(1+x)$ errors are more probable and hence will occur more frequently than with a larger $(1+x)$. And the law that regulates the probabilities of the deviations by various degrees from the average (*i. e.*, the frequency of error with various stimulation ratios $(1+x)$) is the law expressed by the "probability curve," which pictures the effect of a very large (strictly infinite) number of small causes no one of which has of itself any decided influence. And here we have touched bottom. This law forms the basis of the method of right and wrong cases, and enables us to predict what ratio of errors will occur with any value of $(1+x)$ when we have experimentally determined in a given case this ratio, n, for a given value of $(1+x)$.[1] The formula for doing this, together with illustrations of its application, is given in Appendix C.

As the object of any psycho-physic method is to measure the sensibility, it remains to show how this is to be done, and thus to supply the want which the just observable difference was intended to meet: namely, to afford a ready method of comparing the variation of sensibility in different individuals, at different times, with different modes of judging, in

[1] The paper by Mr. C. S. Peirce and the writer, above referred to, illustrates the close correspondence between theory and practice in this respect. The only difficulty in showing this arises from the fact that the probable error is constantly decreasing (due to practice and so on), and one must therefore divide the results into groups within which the probable error is presumptively tolerably constant. This consideration must also be taken into account in calculating an n for a certain $1+x$, on the basis of several n's obtained with several $(1+x)$'s.

different senses, and so on. The frequency of error with each value of the ratio $(1+x)$ is expressed by a continuous curve when $(1+x)$ changes gradually; there is no characteristic point on the curve evidently appropriate for the standard of sensibility, hence the choice of such a point must be made on grounds of convenience and simplicity. The standard of sensibility that I now propose is that ratio of the two stimuli (or rather that ratio less one) with which one half of the answers being correct by chance, one half of the remaining one half of the answers will also be correct—*i. e.*, when *one* error occurs in every *four* answers. The reason of this choice is that this ratio measures the probable error, or that error which is as likely to be exceeded as to be fallen short of. This will be fully explained in considering the method of the average error, and it will there be shown that this standard of sensibility forms the easiest possible transition between the method of right and wrong cases and that of the average error; which, I take it, is an essential requisite of a standard of sensibility. Of course it is not necessary that this value be experimentally found; it is to be calculated by the formula given in Appendix C, from any ascertained ratio of error with any ratio of stimuli. If several such data are at one's disposal it is to be calculated from each, and the mean drawn or treated in what is recognized as the fairest manner.

Having thus obtained a standard of sensibility, it only remains to illustrate its application and to mention some practical conditions which this method makes advisable. I will call that ratio of excitation with which errors occur once in four times the "*standard ratio.*" If, for example, I find as the

result of 1000 experiments with two weights 200 and 210 grams, that 250 of the answers are wrong (or calculate from an equivalent set of experiments that at this ratio of stimuli that proportion of answers would be wrong), then the sensibility of the pressure sense in this case is $\frac{1}{20}$. (The stimuli being 200 and 210, $1+x=\frac{210}{200}=1+\frac{1}{20}$. $\therefore x=\frac{1}{20}$). If in a following series of experiments I find 250 mistakes when the stimulation values are only 200 and 208, then the sensibility has improved from $\frac{1}{20}$ to $\frac{1}{25}$; and thus the sensibility is said to be twice as fine (not when half the ratio of errors are made, but) when the ratio of stimuli necessary to produce the same ratio of errors is halved. If in a given case I find that A makes fewer errors than B, I have only to calculate the standard ratio for each in order to quantitatively ascertain the ratio of their sensibilities, which are inversely proportional to their standard ratios. And finally, I can compare different senses on the generally admitted supposition that their sensibilities are to be measured by the *ratio* of the stimuli (apart from their absolute value) leading to equal ratios of error. This comparison would lose much of its significance and validity in case Weber's law does not hold; for then no such method of comparing entirely different senses would exist, inasmuch as the absolute value of the stimulus would then be important and there is no connection between an ounce and an inch. If Weber's law is true within limits, the comparison holds only within those limits.

If the plan of experimentation thus far sketched were followed there would be little room for serious error, and the experiments of various observers would be generally comparable. I will, however, add some suggestions and precautions, all of which have proved themselves highly advisable, if not essential.

(*a*). It should be stated what knowledge the subject has of the conditions and purposes of the experiment, and the subject should know all the conditions except such as will lead to the use of indications towards forming a judgment other than those furnished by the sensation itself. If he is in doubt as to the several changes that can possibly occur he will infer them for himself, and will yield to that uncontrollable psychological guessing of what is coming. This mischievous tendency plays havoc with the expectation and throws the attention off the track. When the confidence is low the tendency to prefer one kind of answer is apt to occur, and would be avoided if the subject knew that this tendency had no basis in fact. It is especially necessary for the subject to know that in each case the one stimulus is greater or less than the other (and never equal to it), and that either is as liable to come first as last.

(*b*). The greater sensibility for an increase than for a decrease of sensation must be taken into account. This simply means that as a matter of fact one is more apt to perceive a change from 20 ounces to 21 ounces than a change from 21 ounces to 20 ounces, and therefore the two experiments should not be placed on a par. This caution is quite usually observed; but what seems to me the easiest method of avoiding the difficulty is used, as far as I know, only in the experiments on "Small Differences of Sensation" above referred to. It consists in having one of each kind of change in each experiment. For example, in the above case the order of the weights would be (1) 20, 21, 20, or (2) 21, 20, 21, the subject being required to decide whether the middle stimulus was greater or less than the first and third stimulus. The mean of two sets in one

of which only "increases" and in the other only "decreases" are used is very good, but multiplies the number of experiments without in general yielding any compensating advantages. Cases may arise, however, in which the first mentioned process is inapplicable; but these are rare, and in general the "double process" is advisable. It gives two chances of judging and makes the conditions highly favorable. It should be distinctly stated which method is used. In either method one half the answers will be right by the action of chance.

(*c*). It is highly advisable to have as many of one kind of change as of the other, *i. e.*, as many "decreases" followed by "increases" as *vice versa*, or, if the other method is used, as many "decreases" as "increases." This is generally conceded; and the only point worthy of mention is that one can avoid the subject's taking any unfair advantage of this fact by having a large number of experiments in one set. If that is impracticable, divide a large set composed of an equal number of each kind of change by a *chance* arrangement into smaller groups. In general let a *chance arrangement* (die throwing, etc.) decide the order of the several kinds of changes.

(*d*). A precaution that I have found of great value is that the moment at which the change is to occur shall be under the control of the subject, and not, as is usual, at the command of the experimenter. In this way the subject knows exactly *when* to expect the sensation, and he can ask for it at the moment when he is best prepared to receive it. Any slight non-distracting movement can be agreed upon as the signal to mean "change." This is a greater advantage than would at first sight appear.

(*e*). It is hardly worth while adding that the method should be the same throughout, that the conditions be kept as equable as possible, that the effect of practice be noted and of fatigue avoided, and so on and so on.

It will be worth while illustrating by a single example, to what kind of work the employment of a wrong method leads. The author in question is experimenting with the pressure sense.[1] With a constant initial weight of 10 grams he successively increases the differential weight from .1 gram up to the point where, in a set of 16 trials, no wrong answers occur. He thus uses the so-called method of the just observable difference in its worst form, and arbitrarily fixes the point of no error at no error in 16 answers. Not satisfied with this, he in some cases allows an error or two and still calls it the just observable difference. Again, he has three pairs of changes; beginning with 10 grams he either (1) increases, (2) repeats it (equal), or (3) decreases it. He thus makes it impossible to know the number of answers correct by chance. The answer when the change was an increase or decrease may be correct or doubly wrong (*conträr*); when the weights are equal it may be correct or wrong (*falsch*). What happened when the subject said the weights were equal but they were not so is not recorded. Finally, the subject could always say, if he chose, that he was undecided (*unbestimmt*). The object is now to increase the differential weight until only "correct" answers remain, or nearly so. With differences of .1, .3 and .5 gram the *whole table* conveys the important information that the subject never felt like answering at all. With .4 gram he begins to answer,

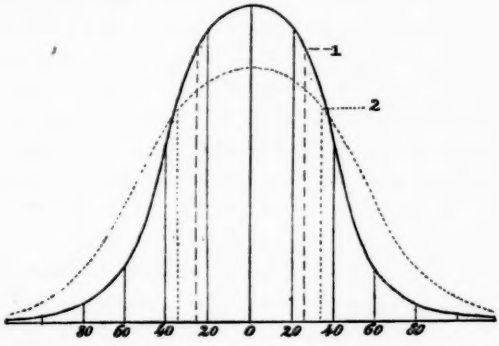
[1] *Experimentelle Prüfung der zu Drucksinn-Messung angewandten Methoden*, etc. Von Dr. Bastelberger. (Eine von der Universität Strassburg gekrönte Preisschrift.) Stuttgart, 1879, pp. 70.

frequently makes mistakes, and at last when the 10 grams are increased by 4.5 grams, or the ratio is nearly 2 : 3, he commits no errors in a set of 16 trials. In this process there were 192 experiments, in *only* 68 of which did the subject give a definite answer. Up to the very last "doubtful" answers occurred. Results like this are utterly unreliable. They show nothing, and are not comparable with the results of others. What has been done is to show that in a particular set of 16 experiments the subject was disposed to answer each time and made no errors, and that with small differences the confidence is low. It is certainly not to be supposed that if the experiments were repeated one would get the same number of doubtful, of right, of singly wrong, and of doubly wrong answers, with the same ratio of stimuli ; and if they differ, we may not be able to tell whether the change is an improvement or a deterioration. Again, the results say that when I put 10 grams on the finger and then take it off and replace it I will not know that the same weight has been replaced, if this occurs in a series when the greatest change to be expected is an addition or diminution of .1 gram, but that I will recognize this *very same* act when it occurs in a series in which changes of .4 gram are made. This difference can only be due to contrast with the preceding and the expected sensations, and is therefore an effect which interferes with an accurate determination of sensibility. And all this is simply the effect of allowing "equal" answers. In short, a correct method would yield results which included these and said much more in half the time.

Critique of Method III.

We come at last to the most natural and in some respects the most important method, that of the average error. This, like the former, is founded upon the probable error; in fact, the x of the standard ratio is the probable error. To show what elements are involved in this method and how they are dealt with, let us take a particular instance. Let the problem be to draw lines equal in length to a given line; in so doing the average result will be (1) to draw a line really equal to the original line, *i. e.* the several exaggerations and under-estimations will balance one another; or (2) to draw a line slightly longer, or (3) one slightly shorter, than the original line. In either case let the point marked zero represent the average result of the reproductions; the points 20, 40, 60, etc. to either side denoting reproductions differing from the average by 20, 40, 60, etc. per cent of the average respectively. Now in any extended series of such experiments the number of reproductions of nearly the average length will be larger than the number of reproductions differing more from the average. And the law which this grouping about an average follows is that expressed by the probability curve. In other words, that curve pictures the frequency of each degree of fluctuation to which the judgment is exposed, the abscissae measuring the extent of the error in each direction, and the ordinates the ratio of errors of each degree of error. The *average error* of these adjustments is obtained by dividing the sum of the deviations from the average (without regard to sign) by the total number of adjustments; the *probable error* is obtained by simple mathematical processes (explained in Appendix D, *q. v.*), and measures the

limits within which any observation is as likely to fall as it is to fall beyond them. It is that point on the curve the ordinate from which divides each half of the curve into two equal areas, and is thus represented by the dotted ordinates.



This probable error is the gauge of variation in sensibility from day to day, in different individuals, and so on. The point which it marks is chosen mainly for convenience and the simplicity of the formula to which it leads.

Let me illustrate how the probable error measures the sensibility. If to-day I am in a better judging condition than I was yesterday, my probable error will be less ; this means that I will be more consistent, be less subject to large disturbing variations, and react more nearly in the same way on each occasion. This is what is indicated by the probable error, and (as we saw in the method of right and wrong cases) is what we do and ought to mean by being able to judge better. Again, if A is a better observer than B, the complete significance of this fact is expressed by saying that his

probable error is less than B's; this is fully recognized in astronomical and other exact observations. It is not an ultra-refinement, but is at once the simplest, most accurate and adequate mode of expressing those differences with which psycho-physics deals. Finally, to say that one sense is finer than another is to say that its probable error is less. For example, the sense of vision is finer than the pressure sense; this means that if I repeatedly select from a large number of slightly different weights one that shall equal a given weight, the point in each half of the curve which has an equal number of errors to either side of it will be farther off in the pressure curve than in the curve resulting from matching two lines by the sense of vision, as shown in the figure. Moreover, the probable error furnishes a quantitative estimate of sensibility. If A has twice the sensibility of B this means that his probable error will be one half that of B. If the effect of practice is to increase my sensibility by one half its first amount, my probable error will decrease by one third of its amount, ($\frac{1}{3} = \frac{2}{3}$; $1 - \frac{2}{3} = \frac{1}{3}$).

Let us return for a moment to the three possible results of the method of the average error (or, as we have just seen reasons for terming it, the probable error) as above given. If the average result of all the adjustments equals (or nearly equals) the real intensity of the first stimulus, it shows that the causes leading to error in one direction are equal in efficiency to those causing errors in an opposite direction. If, however, the average result of all the adjustments shows a constant deviation (either greater or smaller) from the original stimulus, then there is a constant and a variable error, which two are totally different and independent things. The constant error must be ascribed

to some peculiarities of our organism and so on, and has no value whatever in measuring the sensibility; this, as before (and always), is measured by the probable error of the deviations of the several adjustments from their mean. The constant error measures something very important and forms a special object of research. But, it will be asked, how will this constant error appear in the method of right and wrong cases? As regards the ratio of error it will not appear at all. This constant error would appear in the curve as a shifting of the central axis to one side. This does not affect the probable error, which alone decides the ratio of error in the method of right and wrong cases. Why it does not thus appear may be seen from the following considerations. The constant error makes the probability of a certain deviation—inasmuch as that deviation is made larger by the existence of the constant error —*less* than if no such error existed; but this is exactly counterbalanced in those equally frequent cases in which the constant error aids to the same extent in *lessening* the degree of a deviation. For example, if the effect of the constant error is to lead me to regard a line $\frac{5}{4}$ of the first line in length as its equal, then in the method of right and wrong cases this means that the probability of my making an error of any degree is made less because I must now make an error $\frac{5}{4}$ of its size; this is when an increase is taken to be a decrease. But when I mistake a decrease for an increase, the additional $\frac{1}{4}$ to the length of the line by that much *decreases* the size and increases the probability of such an error. But if the effect of the constant error is such as to always make the altered (not the initial) stimulus seem larger, then the constant error will appear in the fact that more errors in taking a decrease to be an

increase than *vice versa* will occur; and in fact we can quantitatively determine the constant error by taking half the difference between the probable error of all the judgments of one kind and that of the other kind of judgments—a proceeding which I do not remember to have seen in practice.

There are no special precautions necessary in carrying out this method. It is natural and easy, but not practically applicable to all senses. One must take care that the subject really has a free choice of all such reproductions as he is at all likely to choose.

Conclusion.

It will have been noticed that this critique has dealt solely with the theoretical and practical justifications of the three usually recognized psycho-physic methods. It has avoided any reference to the psychophysic law in Fechner's sense, and only in a few places has it been led to consider Weber's law. Weber's law is either (1) true throughout the psychic scale, or (2) it is not true at all, or (3) it is true within limits. In the second case, as has been noticed, we lose a valuable method of comparing the accuracy of different senses unless a law similar to that formulated by Weber can be proved to hold. In the third case we must limit our comparison of different senses to those absolute stimuli which show the greatest tendency to be in accord with Weber's law. The question of a practical correction for the lower and upper end of the sensitive scale in each sense is a separate one, and cannot be considered here. My object now is to point out that a main function of Weber's (or any similar) law is to supply a method of comparing the sensibility of different senses, and the

function of the two legitimate psycho-physic methods is to furnish standards of sensibility in the several senses. The results obtained by either of these methods can be expressed in terms of the other. The difference between them is in the psychological processes of which they make use; and it is possible that this difference is so great as to some slight extent to vitiate the mathematical relations that have been deduced for transition from one to the other. This can only be decided by actual experiment; and such experiments, if sufficiently numerous and carefully conducted, would form a valuable contribution to the subject. If the result were to show an agreement between theory and practice (as I believe it would), it would give an especial significance to the definition of man as a rational animal.

Finally, a word as to Fechner's law, which reads that the sensation is proportional to the logarithm of the excitation. That law in one sense, I believe, can be deduced from Weber's experiments only by the use of a series of assumptions, hardly one of which is even probably justifiable. Fechner has confused "the sensation of being different" with "the difference of sensation," and his law seems to me, in the sense in which it is often, if not usually, stated, to be without truth or meaning. But I reserve all criticism of this as well as of other fundamental propositions in the logic of psycho-physics for another occasion, and will conclude this paper with a summary of the main points which have been advanced therein.

(1). The method known as the method of the just observable difference is either not at all suitable for an exact measurement of sensibility, or it is but a loose application of the method of right and wrong

cases. It should therefore be omitted from the psychophysic methods, where it has introduced much confusion and many misconceptions.

(2). The threshold is such a misconception, arising from a discrete mode of regarding continuous quantity; and is as valueless as a standard of sensibility as it is unjustifiable theoretically. The variations of the probable error form a continuous curve, while the threshold theory requires a more or less sudden change in the direction of this curve.

(3). The method of right and wrong cases is justifiable when used with certain precautions; in particular, when but two answers are possible and but two kinds of excitation are used; when the subject is required to record a definite answer each time; when the number of answers correct by chance is known (and equals one half). Other advisable rules are given in the text.

(4). The justification of this method lies in the fact that the causes of error follow the probability curve; and thus a means is furnished of calculating either the ratio of errors at any given ratio of stimuli, or the ratio of stimuli at any given ratio of error, when the ratio of errors at any one ratio of stimuli is known.

(5). The standard ratio by which sensibility is to be measured is that ratio of stimuli at which one error occurs in every four answers.

(6). The method of the average error (better, of the probable error) depends directly on the ascertaining of the probable error; and the probable error itself measures the sensibility. The x of the standard ratio in the method of right and wrong cases is the probable error, and this fact yields a ready method of comparing the results of the two methods.

(7). The function and value of Weber's law depends

on its furnishing (it may be within limits) a means of comparing the sensibility of different incommensurate senses. It can be formulated in terms of the method of right and wrong cases, as saying that the standard ratio is independent of the absolute value of the stimuli but depends solely on their ratio $(1+x)$; and in terms of the method of the average error, as saying that the probable error will be uninfluenced by a change in the absolute size of the stimulus according to which the adjustments are to be made.

Appendix A.

The Practical Threshold.

While I maintain that the theoretical refutation of the threshold theory and the establishing of the point of view of the probable error carries with it the assurance that no practical difficulty to which they may give rise will be more than an apparent one, yet it may be worth while showing how such objections are to be met. The favorable evidence which the assumption of a threshold derives from ordinary experience can be illustrated thus: We do not see the stars at day, yet they are there. This can only be because the lustre added by their brightness to the enormous sunlight already existing is too insignificant ever to appear visible to our eyes; it is lost below our differential threshold. In so extreme an instance the difference between the current view of the threshold and the one here advocated becomes theoretical only; but that does not lessen its importance. Consider the facts more closely; at day the star is invisible, at night it is visible. Hence, the argument reads, there must be a point where the visible passes into the invisible at dusk and comes back into the visible again

at dawn. The question is, what is the correct mode of describing this process. The current method is this: the ratio of the brilliancy of the star to the already existing light is constantly increasing, and when this ratio has increased beyond a certain amount (the differential threshold for vision) the star becomes visible. My explanation would be this. I would first call attention to the fact that the star would be invisible to some persons when it is visible to others, would under parallel conditions be invisible to me one day at a given time and visible the next day, in order to show that the term threshold is intended to refer to an average threshold. I would then ask whether you will always be able to see the star a minute time after the ratio of its brilliancy to that of the sun has increased above the ratio referred to. If you answer "yes" you define your threshold to mean that ratio of the brilliancy of the star to the sun at which all your answers will be correct. Here you either (1) tacitly assume that not many observations are to be taken, or that (2) no matter how many observations were made no mistake would ever occur. If you mean the former you admit that if the observations went on errors might occur; but the causes which led to these errors have not totally vanished, but have only gradually decreased without any sudden break in the process—*i. e.* without any threshold. If you mean the latter you are claiming a very improbable proposition; for the causes leading to error still exist, and though very minute, and errors rare, still they are never impossible. *Practically* they will be impossible after a certain more or less definite point; but this simply means that it would be impracticable to collect sufficient observations to ensure the occurrence of an error. One can agree to mean by a *practical* threshold that ratio

of excitations at which no more than one in a hundred or one in two hundred answers will occur—*i. e.* one can agree to neglect all causes of error not sufficient to produce at least one error in one or two hundred trials; and can use this as a standard ratio, to be calculated as the other standard ratio. But reasons have been given for preferring the standard ratio first proposed. If, however, a practical threshold be desired, it can be agreed upon, but it will not be a real threshold in any true sense. The star would be far below such a practical threshold.

I am indebted to Dr. Fabian Franklin, of Johns Hopkins University, for pointing out that there is a form of the threshold theory consistent with the mathematical basis here advocated. It is this: we can imagine a ratio of stimuli differing very slightly from unity which a judgment less subject to fluctuations than ours would (owing perhaps to some peculiarities of its organism) more often disregard. And the more perfectly free from fluctuations such a judgment is, the more automatic the process of judging, (not the more often will this small difference be perceived but) the more often will it *just fail* to be perceived. We are dealing not with more and more observations but with a better and better judgment. And as this judgment approaches perfection we can imagine it perfectly perceiving certain differences and perfectly failing to perceive all differences below a certain fixed difference, which would thus be the threshold.

In reply to this I have only to state that (1) from the experience that we have we can assert that such a state of things is extremely improbable, and (2) that if it were true it would necessitate the same psycho-physic methods which are here considered valid, and that in

brief the practical outcome of it would be quite the same as those that arise from the theory here advocated. It might be worth while devising experiments to test the possibility of this supposition. It is to be noted that this form of the threshold theory is as antagonistic to the old threshold theory as the one advocated in this paper. Such a threshold must be very much more minute than any value assigned as the differential threshold in the old sense.

APPENDIX B.

The Method of Gradual Increment.—The Confidence.

In discussing the method of the just observable difference it was implied that though the usual method of that name was not valid, there was a genuine form of the method. In its true form it has recently been applied to the study of the pressure sense.[1] It consists in allowing the initial weight to change *gradually*, and to find *when* the subject detects the direction of the change—whether an increase or a decrease of pressure.

A study of the nature of this proceeding sheds much light on the operations involved in the process of judging. The sensation gradually changes, and the question is how soon is this change detected? In the first place it is to be noted that there are two variables, the rate of change and the amount of change. For the sake of simplicity suppose the rate of change constant. By how much must my sensation change before I am willing to decide in what direction it has changed? My point is that this is to a large extent an individual matter. It means what is the smallest amount of confidence upon which I will risk a judgment. If I wait until I feel perfectly certain about it

[1] See the article by Hall and Motora in No. 1 of this Journal.

my "just observable difference" will be large; if I judge as soon as I have a minimal amount of confidence I will have a small "just observable difference," but will doubtless make many mistakes. This feeling of confidence is what the "just observable difference" method takes into account. And we would expect that the ratio of errors in the method of right and wrong cases varies in an opposite direction not only with the difference judged, but also with the confidence in the correctness of that judgment. When the difference of the stimuli is constant the number of errors in the various sets will vary "inversely" as the confidence; and hence this subjective feeling may be utilized for recording the differences between individuals and between different series of judgments of the same individual. The feeling of confidence will itself be liable to variations, but every one will doubtless have a tolerably constant "index of confidence."

We see thus that in its true form the method of the just observable difference measures the disposition to answer, and this in turn is determined by the subjective feeling of confidence. The method is calculated to shed much light on the subjective states that accompany the act of judging, but though valuable in other directions, is not suited for measuring sensibility. It is also to be noted that as a considerable variation in the confidence from time to time, or even between different individuals, is not to be expected, that feeling of confidence which prompts one to answer may be considered sufficiently constant to enable one to base a rough measurement of the sensibility upon it and not upon the correctness of the answer. And in this way the just observable difference as ordinarily tested may be useful in hurriedly

testing sensibility, in pathological cases and elsewhere. What I have said is not opposed to such a use of it.

To record the confidence is a difficult and must to a large extent be an arbitrary matter. In the experiments on "Small Differences of Sensation" we used the following plan: 0 denoted the absence of any preference for one answer over its opposite, 3 denoted as strong a confidence as one would have in ordinary sensations, and between the two 1 and 2 naturally found their places. From records made on this plan Mr. Peirce deduces the formula $m = c \log \frac{p}{1-p}$, where m denotes the degree of confidence, p the probability of the answer being right, and c a constant which may be called the index of confidence. This formula closely approximates the results actually obtained. It appears, too, that with increased practice the index of confidence rises.

It was above deduced that the confidence must vary in a direct sense with the ratio of the stimuli, and in an opposite sense with the ratio of errors. This is very clearly shown in our experiments. Mr. Peirce's average confidence was .67 when the two stimuli were 1000 and 1060 grams; was .28 when they were 1000 and 1030 grams; and was .15 when they were 1000 and 1015 grams. Similar numbers for myself are .90, .51 and .30; and when the ratio of the stimuli was further diminished my confidence was still further reduced. With the stimuli 1000 and 1005 grams it was practically zero. In a paper published by the writer in *Mind*, No. 44, similar results are shown.

Again, the confidence varies in an inverse sense

with the number of errors. In Mr. Peirce's case only 3 per cent of all answers given with a confidence 3 were wrong; 10 per cent of those with a confidence 2; 18 per cent of those with a confidence 1; and 38 per cent of those with a confidence 0. Similar numbers for myself are 3, 6, 16 and 30 per cent. In spite of the obvious arbitrariness and inadequacy of this method it has proved itself surprisingly useful; it ought, however, to be improved in future work.

APPENDIX C.

Formulae for the Method of Right and Wrong Cases.

For these formulae as well as for important suggestions in several parts of this paper I am indebted to Dr. Fabian Franklin.

I. Rule for calculating the ratio of the two stimuli at which one fourth of the answers will be wrong when the ratio of wrong answers at any one ratio of stimuli is given.

Let $(1+x)$ be the given ratio of stimuli; let $(1+p)$ be the ratio at which one in four of the answers will be wrong; and let n be the ratio of errors with the ratio of stimuli $(1+x)$. The formula is

$$\log(1+p) = \frac{.477 \log(1+x)}{\theta^{-1}(1-2n)},$$

in which $\theta^{-1}(1-2n)$ means the t in a table of θt corresponding to $\theta t = 1-2n$. Such a table is here appended and is taken from the article on Probability in the Encyclopedia Britannica, 9th edition. (It is to be noted that as the logarithms appear finally as a ratio they may be taken in any system of logarithms.)

Example 1.—In distinguishing between what weight and 100 ounces would A answer wrongly once in four

times, if he makes 15 errors in 100 answers when distinguishing between 100 and 105 ounces.

$$n=.15\,;\ 1-2n=.7\,;\ \theta^{-1}(1-2n)=\theta^{-1}(.7)=.7345\,;$$
$$1+x=1.05\,;\ \log(1.05)=.0212.$$

$$\log(1+p)=\frac{.477\log(1+x)}{\theta^{-1}(1-2n)}=\frac{.477(.0212)}{.7345}=.01377\,;$$

$1+p=1.032$. *Answer:* Between 100 and 103.2 ounces.

Example 2.—If in distinguishing between the brightness of two screens, the illumination of one of which is brighter by $\frac{1}{50}$ than the illumination of the other, B errs on the average 19 times in a set of 50 observations; what ratio of brightness must the second screen bear to the first for B to make only 12.5 wrong answers on the average, in a set of 50 observations?

$$n=.38\,;\ 1-2n=.24\,;\ \theta^{-1}(1-2n)=\theta^{-1}(.24)=.2164\,;$$
$$1+x=1.02\,;\ \log(1.02)=.0086.$$

$$\log(1+p)=\frac{.477\log(1+x)}{\theta^{-1}(1-2n)}=\frac{.477\,(.0086)}{.2164}=.01896.$$

$\therefore 1+p=1.0445$. *Answer:* The ratio $\frac{209}{200}$ (nearly).

II. One can find the ratio of stimuli at which *any ratio of errors* will occur when the ratio error with a given ratio of stimuli is known, by the following formula: With the formula $\log(1+p)=\frac{.477\log(1+x)}{\theta^{-1}(1-2n)}$ find the value of p; then with this value of p and the designated new value of n, find the value of $(1+x)$ by the same formula transposed, viz.

$$\log(1+x)=\frac{\log(1+p)\,\theta^{-1}(1-2n)}{.477}\ .$$

Example 3.—In Example 1, with what ratio of stimuli

will A make only 7.5 errors in the average set of 100 observations?

We had $p = .032$ or $\log(1+p) = .01377$, and $\theta^{-1}(1-2n)$ will now be equal to $\theta^{-1}(.85) = 1.02$.

$$\log(1+x) = \frac{(.01377)(1.02)}{.477} = .0295, \text{ and } (1+x) = 1.07.$$

Answer: 1.07.

III. One can also find the ratio of error at *any* ratio of stimuli when the ratio of error with one ratio of stimuli is given, by the following formula. Find $\log(1+p)$ as before. Then find n in the following formula where $(1+x)$ represents the new ratio of stimuli,

$$n = \frac{1 - \theta\left\{\frac{.477 \log(1+x)}{\log(1+p)}\right\}}{2}$$

Example 4.—Find the ratio of wrong answers in Example 1 when the ratio of stimuli is 1.1.

We have $\log(1+p) = .01377$; $\log(1+x) = \log(1.1) = .0414$. Hence

$$n = \frac{1 - \theta\left\{\frac{.477 \log(1+x)}{\log(1+p)}\right\}}{2} = \frac{1 - \theta\left\{\frac{(.477)(.0414)}{.01377}\right\}}{2}$$

$$= \frac{1 - \theta(1.43)}{2} = \frac{1 - .956736}{2} = .021632.$$

Answer: 2.16 errors in 100 answers.

IV. I will also show how a practical threshold can be obtained if desired.

Example 5.—Taking the practical threshold at one error in 100 answers and the probable error within its extreme limits in the case of the writer in the experiments on pressure above referred to, viz. .05 and .016, at what ratio will this threshold occur?

$$\log(1+x) = \frac{\log(1+p)\,\theta^{-1}(1-2n)}{.477},$$

$\log(1.05) = .0212$; $\log(1.016) = .00689$;
$\theta^{-1}(1-2n) = \theta^{-1}(.98) = 1.649$,

$$\log(1+x) = \frac{(.0212)(1.649)}{.477} = .07333; \quad \therefore (1+x) = 1.184,$$

$$\log(1+x) = \frac{(.00689)(1.649)}{.477} = .02383; \quad \therefore (1+x) = 1.056.$$

Answer: 100 ounces and 105.6 ounces in the first case; 100 and 118.4 ounces in the second case.

Table of θt from $t=0$, to $t=3.0$.

t	θt	t	θt	t	θt	t	θt
0.00	0.00000	.2	.22270	1.3	.93401	2.4	.99931
.01	.01128	.3	.32863	1.4	.95229	2.5	.99959
.02	.02256	.4	.42839	1.5	.96611	2.6	.99976
.03	.03384	.5	.52050	1.6	.97625	2.7	.99986
.04	.04511	.6	.60386	1.7	.98379	2.8	.99992
.05	.05637	.7	.67780	1.8	.98909	2.9	.99996
.06	.06762	.8	.74210	1.9	.99279	3.0	.99998
.07	.07886	.9	.79691	2.0	.99532	∞	1.00000
.08	.09008	1.0	.84270	2.1	.99702		
.09	.10128	1.1	.88020	2.2	.99814		
.1	.11246	1.2	.91031	2.3	.99886		

Note.—Intermediate values in this table are derived by interpolation in the ordinary way.

Appendix D.

Rules for Computing the Probable Error.

These rules I take from Jevons, Principles of Science, p. 387.

1. "Draw the mean of all the observed results.

2. Find the excess or defect, that is, the error in each result from the mean.

3. Square each of these reputed errors.

4. Add together all these squares of the errors, which are of course all positive.

5. Divide by one less than the number of observations. This gives the *square of the mean error.*

6. Take the square root of the last result; it is the *mean error of a single observation.*

7. Divide now by the square root of the number of observations, and we get the *mean error of the mean result.*

8. Lastly, multiply by the natural constant 0.6745 (or approximately by 0.674, or even by $\frac{2}{3}$), and we arrive at the *probable error of the mean result.*"

For illustrations of this process and methods for shortening the work see Jevons and works on "Probabilities" there referred to.

It is generally advisable to divide up the observations, and find the probable error of each group and then draw a mean. It is also sometimes desirable to be able to test how closely the number of errors of each degree of deviation from the mean follows the number assigned by the probability curve. Mr. Francis Galton gives an admirable account of this in an appendix to his "Hereditary Genius," to which the reader is referred.

PSYCHOLOGICAL LITERATURE.

I.—EXPERIMENTAL PSYCHOLOGY.

Zur Psychophysik des Lichtsinns. Von HJALMAR NEIGLICK. Philosophische Studien, IV, 1, pp. 28–112.

This is a continuation of the experimental study begun by Dr. Lehmann in the former number of the *Studien*. The method used is that of the "mean gradations," and consists in rapidly rotating three discs, each containing a certain amount of black and white, so that in rotating a uniform gray of a lighter or darker tint is produced, and in requiring the observer to regulate the amount of black on one of these discs so that it shall produce a gray exactly intermediate between the constant grays of the darker and the lighter discs. If the amount of black on the adjustable disc proves to be the *mean proportional* between that on the light and that on the dark disc, Weber's law holds.

Lehmann's elaborate study brought out the many sources of error in this experiment, and above all, the enormous effect of the contrast of the disc with its background. It was found best to set each disc against a background of its own tint; this can readily be done for the two constant discs, but seems difficult to do for the medium disc without giving the observer a clue as to the tint he ought to choose. Neiglick solved this problem by having the background itself a disc much larger than the one to be adjusted, but similarly marked as to white and black, so that when both rotate on a common axis, the adjustable disc, like the others, is seen against its own background. With all these precautions it was found that in a general way Weber's law held, and seemed to hold the more rigidly the more carefully the experiment was conducted. But a new result, on which Professor Wundt, in a note to this article, lays much stress, is that the absolute difference in grayness between the extreme discs affects the validity of the law: in other words, while the mean proportional between x and y is $\surd xy$, and the mean proportional between $\frac{1}{2}x$ and $2y$ is also $\surd xy$, yet, as a fact, the adjustment of the one pair will be nearer the mean proportional than that of the other pair. And the difference between the discs in which the law has its greatest validity corresponds to that relation of the tints of the two discs at which the researches of Lehmann showed that the maximum amount of mutual contrast occurs. For example, a setting in which the one disc is entirely white and the other 40° of black is one of the relations at which the law most closely holds. An interesting discussion of the bearing of the phenomena of contrast on Weber's law closes the article.

Two remarks may be added to the account of this research: the first is that it proves the extreme intricacy of this psychophysical method, and yields an excellent instance of the way in which side effects can entirely distort the law of a series of phenomena; the

second remark is that, important as these results are, the author has no right to subject one to the reading of 84 pages to winnow them out. This lengthiness is a fault common to many of the studies from the Leipzig laboratory. J. J.

Zur Theorie der Gesichtsempfindungen. Von J. v. KRIES. Arch. f. Anat. u. Physiol., 1887, p. 113.

In a paper published in 1878 Kries stated a law of physiological optics, that lights composed of different colors which seemed alike to the unfatigued or neutral eye seemed alike also to the eye however fatigued, and two years ago he was led to conclude from further experiments that if the physiological effect of two objectively different light-mixtures was identical, the identity remained if the intensity of the lights was increased or diminished in the same ratio. The first of these laws was new, and the second perhaps implied in the broader law of Grassmann and Helmholtz that lights that appear equal give mixtures that appear equal, but has been more recently questioned. More recent experiments by Hering confirm both laws. From the first law Kries has developed an objection to Hering's theory of visual sensation as follows: A light composed of red and green may seem to an unfatigued eye identical with a light composed of yellow and blue. If the eye be now fatigued, *e. g.* for red, the first light ought on Hering's theory to seem greenish on account of the change in his red-green visual substance, while the other light which did not affect this substance would remain unchanged. The two mixtures, however, do remain the same. Hering accordingly modifies his theory, or, as he says, the statement of it, as follows: We must conceive, he tells us, that yellow and blue light are not without effect on the red-green visual substance and *vice versa*, but represent stimuli of dissimulation and assimulation of equal strength. In other words, he adds to his theory the conception that a light may have at the same time on the same substance two opposite effects, and that these effects must be equally intense for all five of his valences. This v. Kries thinks extremely artificial and improbable, as much so as if two chemically distinct substances should give exactly the same spectrum. With more than three components it requires improbable and *ad hoc* assumptions to explain the facts. Again, if a blue and a white light seem equally bright, they cease to do so if the intensity of both is increased in equal relation. This simple fact, says v. Kries, is absolutely irreconcilable with Hering's theory. So is the fact lately placed beyond all doubt by König and Dieterici, that those that are born color-blind fall naturally into two great groups, the red and green blind. Thus the Young-Helmholtz demarkation of components is again confirmed. The advantage of the latter theory is that it apprehends the effects of light so nearly as they are known in photo-chemistry. Since the discovery of the chemical effects of light on the retina, the conception of different substances in the retina has gained ground, and also that their decompositions represent the components of the Young-Helmholtz theory. The fact that the sensations of heat and cold, once thought to represent two opposite processes in the same nerve, is now known by the discovery made independently by Dr. Donaldson in the psycho-physic rooms of this University and by Dr. Goldsheider, now of Berlin, that these two sensations have distinct nerves and terminal organs, destroys the only analogy that supported the theory of Hering, which will be quite abandoned.

Der entoptische Inhalt des Auges und das entoptische Sehfeld beim hallucinatorischen Sehen. Prof. J. HOPPE. Allg. Zeitschr. f. Psychiatrie, Jan. 1887.

The author rejects the theory of centrally initiated and centrifugally projected hallucinations or pseudo-hallucinations in the sense advocated by Kandinsky, and repudiates the term "reflex hallucination" on the ground that hallucination implies consciousness and reflex action excludes it. Hallucinations are defined as involuntary perceptions constructed from internal stimulus of the sensory nerves. This stimulation may be spontaneous by chemical, mechanical, vasomotor, trophic, or muscular action, may depend on the action of subcortical centres, on the entrance of already acquired concepts into the centre, or on the perceptive activity of consciousness. The material of hallucination is the excitation of the peripheral end of the nerves of sense. On falling asleep by day, Professor Hoppe has a sense of growing pressure between his fingers, as if holding a cigar, so vividly that he often looks to see if it is there, and with closed eyes often seems to see it. This spontaneously aroused sense of pressure is the material of hallucination. Sleeping with arm hanging down from a sofa, as the blood pressure increases and the hand seems to close more tightly, the sense of holding a rod becomes so vivid that only the eye can dissipate the hallucination. In the eye nothing in front of the retina can justly be received as material of hallucination. These, and pressure phosphenes, the images of retinal vessels, zigzag figures, the phenomena of contrast and physiological color sensations, are rather to be called illusions. But the pupil, if it be visible, blood corpuscles, the pulsation of the central artery, persistent after-images, and subjective phenomena represent material of hallucination. After-images the original of which has escaped us, and which we may later remember to have seen. But if we cannot do so, the act of perception is the same as if the real objects were before us. Although the entoptic material of the eye is transformed in an hallucinatory sense. Many forms emerge from the macula lutea in entoptic seeing with closed eye, suggesting that it is a seat of memory for images that reach it from without. The writer has repeatedly discovered, after special search in his environment, the originals of strange forms that first entered his consciousness as after-images, but had themselves passed unobserved. Memory consists largely of persistent after-images, and if it is a function of all nervous tissue, may be in part located in the retina, and thus the questionable hypothesis of excentric projection from the cortex be obviated. A long and minute description of the sequence of images, discs, cubes, sand, raindrops, carpet patterns waved by the pulse, clouds that become ships, fields of corn, trees, etc., as observed by the author in his eyes, pronounced normal by an expert ophthalmologist, follows. These are ascribed to circulatory and nutritive processes in the retina, which are also in this case material of hallucination.

Gegenbemerkung "eine neue Urtheilstäuschung im Gebiete des Gesichtssinnes" betreffend. SIGM. EXNER. Pflüger's Archiv, 1887, p. 776.

Three years ago Exner described the following striking phenomena: On an extended background of uniform brilliancy a small field of different hue but of about the same brightness is superposed. If the brightness of the background is changed by a flickering of the source of light, it is the small field, which is really constantly illum-

inated, that seems to flicker, and the larger background appears unchanged. In a longer article in the same *Archiv*, 1886, Hering savagely criticises Exner's phenomena as not new and not illusions of judgment but of sensation, and intimates that Exner has not taken the trouble to study his views, but follows Helmholtz blindly. Exner replies that he himself discovered independently and in another way the central element of Hering's theory of contrast, the influence of one part of the retina by another. While in general a believer in Hering's contrast theory, he finds it inadequate to the explanation of many details.

Handbuch der physiologischen Optik. H. VON HELMHOLTZ. 1886.

The first three *Lieferungen* of an entirely revised edition of this great classical work are received. The first edition, which has long been out of print, was a work of amazing experimental and literary industry and acumen. In the wellnigh a quarter of a century which has elapsed since its appearance, so much work has been done in this field that a thorough revision of it to the end will involve much labor and be of correspondingly great value. We trust nothing will interfere with its completion.

The Dreams of the Blind. By JOSEPH JASTROW, Ph. D. New Princeton Review, January, 1888.

As long ago as 1838 Dr. G. Heermann published an exceedingly valuable study on this subject, concluding from a broad induction that those who lost sight before the age of from five to seven years do not in adult life continue to dream in visual terms as those do who lose sight after this critical period. He also concluded that deafness carried mutism with it before but not after this same period, which was also critical for dream memory of lost limbs. Dr. Jastrow here takes up the general subject on the basis of an examination of nearly 200 blind persons, and while in general confirming Heermann's results, modifies them in essential details and adds much new material in an article of value and interest and with a wide range of suggestive allusion and literary reference. From 100 answers to the question "What is your earliest remembrance of yourself?" Dr. Jastrow found the average age to go back to 5.2 years. At about this age he says there is a declaration of independence of the sense centres from their food supply of sensations. Thus it can no longer be said that when a sense organ is totally destroyed the ideas received by that organ perish too. The writer believes the blind on the whole to dream less than the seeing, but that females dream more than males. Dreams decline from childhood to age, and those of the blind are most likely to be in terms of hearing.

The Writings of Laura Bridgman. By E. C. SANFORD, Fellow of the Johns Hopkins University. Two articles reprinted from the Overland Monthly, 1887.

The valuable reports of Dr. Howe during the most interesting stages of the education of this famous blind deaf-mute are out of print, and Mrs. Lamson did not utilize for her biography the very voluminous journals kept by Laura herself during this period, which Mr. Sanford here has for the first time read through and subjects to a careful analysis which abounds in valuable material too detailed

to be described here. The impressions of the house, furniture, her family, the domestic animals, the family grindstone, the occupations of those about her, her own amusements and childish escapades, impressions of death, etc., all received through the sense of touch alone, and remembered most of them for many years till she learned to write and recorded them, show how independent of language of any sort all the fundamental psychic processes may be. So too the record of the daily events of her life at the Institute, which at certain periods is very full, her so-called poems, her religious impressions, etc., all bear at every point the marks of her defects both in the nature of her impressions and in the structure of her sentences and often her words, but also marvellous success in overcoming these disadvantages. Into Mr. Sanford's analysis of her graphic, syntactical, stylistic and perceptive errors we cannot enter here.

Ueber die optische Inversion ebener Linearzeichnungen bei einäugiger Betrachtung. Von Dr. J. Loeb. Pflüger's Archiv, 1887, p. 274.

An optical figure composed of seven straight lines may look like the contour of an open book and inclined at about the angle at which it would be held in reading, or by optical inversion its middle angle or edge may appear convex to the observer. Loeb tested children of from seven to fourteen years of age, who were told to hold a book as the figure looked to them, and found increasing the distance of the figure excited the concave, diminishing it, the convex, sensation. Absolute distance had nothing to do with the sensation. Even the movement of a pencil, which was not fixated but held between the eye and the drawing, from or to the former caused concave or convex sensations respectively. Slight movements of convergence are commonly associated with convex, and of divergence with concave sensations. Passive movement of the bulbus sometimes caused convergence. Monocular inversion Loeb thinks due to the innervation which changes the fixation point along the line of vision. The same rules hold of all figures susceptible of inversion.

Ueber einseitigen und doppelseitigen Lidschluss. Von O. Langendorff. Arch. f. Anat. u. Physiol., 1887, p. 144.

In man reflex, as distinct from voluntary, winking is always on both sides, but with the rabbit only the lid of the stimulated side winks. The visual field is less identified with the danger field in the rabbit, the eyes of which are on different sides of the head and have different fields, and which needs a strong stimulus to cause bilateral winking. Exactly the same law in man and in rabbits holds of the perfect reflex. Knoll could observe no sympathy of the unstimulated pupil. But it is rare that the voluntary shutting of one eye in man is so well learned that no tremor of the other lid can be observed, and the feeling is that this is due to antagonistic effect rather than to genuine inhibition.

Die Wahrnehmung der Schallrichtung mittelst der Bogengänge. Von W. Preyer. Arch. f. Physiol., 1887, Heft 11 and 12.

To determine how accurately the direction of a short sharp sound could be located with closed eyes and motionless head it was first

needful to determine fixed points in space which could be re-located with accuracy. This was done by means of a wire cap with 26 wires projecting at regular and equal angles in all directions from a point in the head about midway between tympana of the two ears. Each of these directions was carefully named, and to aid in their mental imagery for the subject of experimentation sticks were stuck in a billet of wood in the same directions, and solid wooden figures were made with a side to the plane of which each stick would be vertical. Each time there are thus of course 25 wrong guesses possible, or in all 650 errors. It was found, however, after many thousand experiments, that right and left were very rarely confused, location in the median plane was quite accurately determined, and when errors occurred here neither right nor left ever had preponderance. It was in this plane, however, that the greatest errors, sometimes amounting to 180°, occurred in judging locations front and back. The number and size of errors in the right and left field were surprisingly alike. Preyer assumes that the nerves of each ampulla have a specific energy of localization in space peculiar to themselves. Thus the horizontal canal is strongest stimulated by sounds in the horizontal plane, the upper vertical or anterior by sounds from front and above, and the lower vertical or posterior by sounds from behind and below—each according to its position in the head. That canal is strongest stimulated with the plane of which the direction of the sound (whether through the air and meatus, etc., or through the bones of the skull) makes the smallest angle. These sounds are confused when coming from positions where this angle is nearly alike for two canals. This in general the experiments confirm, although a few positions resist this interpretation. With one ear closed, feeble sounds far over into the field of the closed ear seemed on the side of the open one.

Ueber die Schrift von Schallbewegungen. Von Prof. HENSEN. Zeitschrift f. Biologie, 1886, Heft 3.

Professor Hensen's logograph (Sprachzeichner), the older form of which was described by Grützner in his *Physiologie der Sprache*, has been much improved upon by Hensen of late, and can now be had of his mechanic (Zwickert, Dänische Strasse, Kiel). It now represents better than has ever been done before the impulses which speech imparts to the ear. The curves are very small, but uniform for different pitches, and made by a membrane rigid enough to check after vibrations. That fine curves only a few hundredths of a millimetre long may be made legible and reproducible, it was necessary to warm the glass plate and smoke it over a gas jet so that the coat of soot upon it could just be seen. The mechanical difficulties encountered were great and have occupied Hensen off and on for fifteen years, but now are so far overcome that a pupil of his, Dr. Paul Wendeler, has graphically reproduced a number of consonant sounds with this apparatus, which are described and presented in magnified form in an article following the above. This apparatus seems at least to have one advantage over attempts to write directly from the tympanum or from artificial tympana, or over all such results as Fick has just described (Betrachtungen über den Mechanismus des Paukenfells, Verhandl. d. med. Gesellsch., Würzburg, 1886), in that its *eigenton* is mainly eliminated.

Ueber einige Veränderungen welche Gehörshallucinationen unter dem Einflusse des galvanischen Stromes erleiden. Von Franz Fischer. Arch. f. Psychiatrie, 1887, p. 75.

Jolly, Erlenmeyer and others have recorded cases in which electrical stimulation of the acusticus caused, not a simple sensation of sound, but an auditory hallucination which Jolly thought to be reflex. Since then a closer relationship than was before suspected is held to exist between noises in the ear and auditory hallucinations. Fischer here describes two noteworthy cases in which the galvanic current applied to the central organs caused a change which favored the cessation of auditory hallucinations. The intensification of these hallucinations by chewing, by food (which was therefore refused), the amelioration of them by stopping the ears, the hyperkusia that attended their intensification, and the auditory obtuseness that marked their decline, all point to the same close relationship. Galvanization, it is inferred, however, intensifies psychic excitation unless it is applied when it has already begun to abate, when it is beneficial.

Experimentelle Untersuchungen zur Physiologie des Geruchs. Von Ed. Aronsohn. Archiv f. Anatomie u. Physiologie, III u. IV Heft, 1886, pp. 321–57.

These experiments were made under the direction of Professor Kronecker, and seem well calculated to allay the unusual distrust so commonly felt for subjective sensations in this particular field. On the basis of the old experiments of Tourtual and E. H. Weber, most text-books in physiology state that only gases and vapors, and not fluids, brought into contact with the olfactory organs, can excite the sense of smell. Solutions of salt, wormwood, dilute sulphuric acid, and cologne had been introduced into the nasal cavity. Valentin, and still more recently Vintschgau, after further experimentation, also reached the conclusion that only substances suspended in the air could be smelled. Yet the olfactory organs are covered by a layer of mucous secretion. The common view that fish not only have olfactory organs but use them was further tested by the author as follows: Ant eggs, a favorite food of gold fish, were saturated with clove oil or asafœtida and thrown into a tank, and approached but refused without being touched within several millimetres by the fish. By using a ⊥ tube the author introduced into his own nose solutions of camphor, clove oil, cologne and other substances, with special precaution to avoid injurious degrees of concentration and temperature, and found them distinctly odorous for some time and in more than 100 experiments. A temperature of 40°–44° C. gave best results. Such statements as Kant's, that "smell is taste acting at a distance," or Cloquet's, that "smell is to air as gustatory solutions are to fluids," must therefore stand corrected.

By further experiments it was found that a rinsing solution of about 0.73 per cent solution of salt was most favorable as an indifferent fluid to keep the function of the olfactory organs intact. Reckoning from this as a basis or unity, solutions of other salts of equally favorable degree of concentration were carefully determined and named "osmoteric equivalents." Thus salt has the smallest osmoteric equivalent of the chief fluids of the body. Of the other elements of blood serum, bicarbonate of soda has an osmoteric

equivalent of 2, sulphate of soda 4, phosphate of soda and magnesium sulphate 6. The sensory effects of mixtures of these salt solutions can be correctly calculated on the bases of these equivalents. They have thus their own smell, though they have before been considered odorless.

With the kathode in the nose the author had a distinct sensation of smell in opening an electric circuit, and with the anode in the nose by closing it. In opposition to Bidder it was also found that fragrant substances taken into the mouth and expelled through the choana were distinctly smelled, and it is inferred with Paulsen that the exspiratory and inspiratory current of air take substantially the same course through the nose. Fatigue soon blunts and almost arrests the sense of smell, but it fully recovers its degree of sensitiveness, but not its power of endurance, in a few minutes. Entire exhaustion from one odor leaves the organs of smell with maximal sensitiveness for other odors. Thus the law of specific energy seems to hold for various olfactory fibres or systems of fibres. This fact would seem to give a method by which the much disputed problem of a classification of smells could be solved. But it is needful that experiments be made with chemical substances of known composition. Chemists differ widely respecting the smell of some even of the more common objects, and of many others the text-books do not state whether they smell or not. Only four elements, chlorine, bromine, iodine, and phosphorus, smell. These seem to the author to be odorless in a pure state, and he concludes that all elements are odorless. There are few more vague terms in the psychology of sensation than those designating odorous qualities, and the need of a more chemically scientific nomenclature is greatly felt. Smells were located by the author and others whom he tested not in but before the nose. One of his subjects had a very vivid dream of experimenting with camphor which seemed to be very distinctly smelled. The author finally queries whether the movement of many odorous substances on the surface of water is connected with the ciliary epithelium which Waldeyer lately found over the olfactory region. It seems especially to be hoped that the capacities of the fatigue method of classifying odors will soon be more fully tested.

Note on the Specific Energy of the Nerves of Taste. Studies from the Biological Laboratory of the Johns Hopkins University, Vol. IV, No. I. By W. H. HOWELL, Ph. D., and J. H. KASTLE, S. B.

A chemically pure substance, named para-brom-benzoic sulphinide (formula $C_6H_4Br \begin{cases} CO \\ SO_2 \end{cases} > NH$), first made in the chemical laboratory of this University, and a derivative of the new substitute for sugar called saccharine, was found to cause very intense and pure gustatory sensations of bitter when applied to the back part of the tongue (region of the circumvallate papillae) and a sweet taste when applied to the tip and borders of the anterior half of the tongue. The latter sensation was much feebler, sometimes reported as slightly acid or metallic sweet or slightly astringent. In a few of the twenty persons tested the sensation on the lip was at first slightly bitter, then sweet, which does not accord with the reaction time experiments of Vintschgau, which showed sweet much quicker than bitter. Saccharine itself on the back of the tongue caused in some persons a rapid alternation of the sensations of sweet and

bitter, like rivalry of the two fields of vision. That a chemically pure substance arouses different taste sensations (and those more purely gustatory than acid and salt) favors the doctrine that each taste sensation has its own specifically energized set of nerve fibres.

Die Methode der Aequivalente angewandt zur Maassbestimmung der Feinheit des Raumsinnes. Von Dr. W. CAMERER. Zeitschrift f. Biologie, 1886, pp. 509–559.

The chief object of this "method of aequivalents" (first used by Weber) is to ascertain the relative sensibility of different parts of the sensory surface. Dr. Camerer contributes a very extensive though somewhat unsatisfactory series of observations on the "space sense" of the skin as tested by this method. For example, he places the compass points 4 lines apart (1 line=2.256 mm.) on the forehead, and then finds how far apart the points of a second compass must be to produce a sensation of equal aperture on the lips, and finds it 2.4 lines, *i. e.* the "aequalization ratio" of the forehead to the lips is $\frac{4}{2.4} = 1.67$.

The application of the line is always closely successive, and the variations caused by beginning with an aperture too wide and gradually narrowing, or reversing this proceeding; by applying the "constant" compass first or last; by varying the absolute distance between the compass points, are all worked out in detail. It is also evident that a constant as well as a variable error will come into play. The following table summarizes the results of the first portion of his experiments:

Constant Distances.	1st Series. Forehead to Lip.	2d Series. Forehead to Wrist.	4th Series. Palm to Forehead.	5th Series. Palm to Forehead.	Mean of 4th and 5th Series.	Constant Distances.	3d Series. Forehead to Finger Tip.
4 Lines.	1.668	1.0165	0.972	——	——	0.5	1.051
8 "	1.353	0.9763	1.043	0.982	1.012	1.0	1.055
12 "			1.048	0.996	1.022	1.5	1.044
16 "			1.037	0.989	1.013	2.0	1.033
20 "			1.016	0.985	1.000	2.5	1.028
24 "			1.032	1.003	1.017	3.0	1.025

Each ratio is based upon 240 observations, and the distances were always applied transversely. An important result is that the ratio is affected by the absolute size of the distance applied, the ratio approaching unity as the distance increases.

Many irregularities occur; while in the 4th series the forehead has a finer sensibility than the palm, in the fifth series this is reversed. It is also to be noticed that this method does not show nearly as great differences between the severable parts of the skin as Weber does with the method of "just observable differences."

The individual differences of the four observers who were tested were slight; the effect of practice was quite marked, as shown by a decrease of the aequalization ratio; and the average deviation (variable error) was about 8.5 per cent (in Series 1), it being considerably smaller in the larger distances than in the shorter ones.

A few other questions that were asked were these:

(1) If I have the aequalization ratio of the wrist to the forehead and also of the forehead to the lip, by multiplying the two will I get the same ratio for the sensibility of the wrist to the lip as I would by actual experiment? For the instance just noted this is found to hold, the calculated ratio being 1.4012 and the observed 1.3814. A similar comparison of the wrist, palm, and finger-tip, however, gave a discordant result.

(2) It was found that the sensibility near the median line of a limb or part of the body was very slightly superior to the lateral regions immediately next to it as well as to those farther removed.

(3) On the palm this method, that of the "right and wrong cases," and that of the "just observable difference," were applied to the relative sensibility of the longitudinal to the transverse axis, and all three agreed in making the transverse axis somewhat superior.

Dr. Camerer concludes that the reliability of the method is not clearly made out, and that the assumption of certain constants is necessary to account for the discrepancies to which it leads. An attempt is also made to bring the results into connection with the recent views of Goldscheider, but here again agreement is impossible. The facts must be accepted as such for the present, and their explanation be postponed until more is known of this ever widening field of research. **J. J.**

Untersuchungen über den Fühlraum der Hand. Erste Mittheilung. Von Dr. J. Loeb. Arch. f. die Ges. Physiol., September, 1887.

With body fixed, all points touchable by the point of the index finger of a freely movable hand and arm are called, in imitation of Hering's optical nomenclature, the *tactile space* of the hand. The rectilinear distance between any two points in this hemispherical space is called the *tactile tract*. The *nuclear point* is, arbitrarily chosen, determined as the point in the median plane (between the tactile spaces of the two hands) where the index fingers meet when the upper arm is adducted and the elbows flexed at right angles. In the first series of experiments a horizontal thread was stretched through the nuclear point, and grasped at that point with thumb and finger of each hand. At a signal both hands moved symmetrically out with closed eyes and as nearly equal rate as possible till halt was called. The distance traversed by each hand was measured in experiments on about 30 persons. Each person was found to have a preferred hand which went always farther than the other, the difference being from one tenth to one half the entire tactile tract, and often with an apparent maximum at 150–200 mm. from the nuclear point, from which the experimenters always tried to keep the distance constantly equal for both hands. If one hand was moved passively, neither the sense nor the constancy of the result was affected, yet the tactile tract of the moved hand was very slightly increased. Knowledge of the constant error he was making on the part of the experimenter had only a temporary effect in correcting it. In hospital patients with unilateral defect the asymmetry was greatly increased. When both hands moved at the same time in the same direction the medial tract was always considerably greater than the lateral, but the over-estimation of the medial tract diminishes very rapidly when one hand passes over into the tactile space of the other. When one tract was marked off and felt, and another to be moved over judged to be equal to it, the reproduced

tract was constantly greater or less in different persons with little reference to the position of the pattern line or its direction. The chief ground of judging the distance traversed by the hand is the time occupied by the motion; to judge differentially of rapidity requires much practice. This appeared in testing Vierordt's statement that a point drawn across the hand seemed smaller the more rapidly it was moved, and by drawing threads and wires with different rapidities between the thumbs and fingers of passive hands. Thus if the duration of the impulse and of the movement is the same, rapidity is generally neglected. Equal volitional impulses give rise to the impression of equal rapidity.

Substantially all these results and others were obtained by a different method, which makes the long discussion that closes this article unnecessary, as long ago as early in 1882, and published in the English quarterly journal, *Mind*, by G. Stanley Hall and E. M. Hartwell, under the title *Bilateral Asymmetry of Function*. These observers also showed that the eyes follow the same asymmetric tendency; that there is a constant error, which was measured, in attempting to bring the index fingers into the position which is designated by Loeb as the nuclear point; that there is a constant asymmetry in reaction time, in maximal clenching movements, etc.

Untersuchungen über die Wärmestrahlung des menschlichen Körpers. Von A. Masje. Virchow's Archiv, January and February, 1887.

These extended and valuable researches were made in Zürich, and in part under the direction of Prof. H. Eichorst, and embrace the study of heat radiation in both normal and morbid, especially fever, states, but later pathological studies are yet to be described in detail. The formula of Dulong and Petit, that the heat radiating from a body is proportional to the fourth power of its absolute temperature, does not apply to living bodies, which lack a constant constitution internally and superficially. All formulae agree in making radiation decrease with decrease of heat for constant conditions with lifeless bodies, while for the human body, especially in fever where anti-febrile medicines are used, radiation of heat increases as the body cools. All previous studies, from Scharling in 1849 to d'Arsonval in 1885, followed the same method. A naked man was placed in a receptacle in a room of constant temperature, and after a given time the difference of temperature between the receptacle and the room was made the basis of calculation. This, however, does not show the normal, but rather the artificial loss of heat. The method used by Masje was to allow the heat from any exposed part of the body to radiate through a closed card-box, to avoid air movements, upon a fine metallic electric conductor, whereby its resistance is changed proportionally to the elevation of the temperature. Another equilibrating conductor also, of long strips of tin foil on gutta percha, is used, and between the two is a galvanometer. When the two conductors are at constant temperature and a current is allowed to pass through them, the effect of the two can be so exactly balanced by a rheochord that no deviation of the mirror of the galvanometer is observed. But if one is exposed to the radiant heat of the hand, the resulting difference of temperature in the conductors is very accurately recorded by the galvanometer in excursions directly proportional to the heat absorbed by the conductors. By this method the following results were reached. After uncovering

a part of the body usually covered, the radiation of heat from it increases, not always constantly, but with variations, and this increase is more rapid if the surrounding temperature is low. Parts normally uncovered, as the hand and face, radiate heat about uniformly all times of day. Under the same meteorological conditions of atmospheric humidity and barometric state, radiation in the same person varies from day to day, as does the relative radiation from different parts of the body. Radiation is least on parts of the body covered with hair; it is more on flexor than on extensor sides of the limbs, especially the arms; on symmetrical points it varies but little in adults, but sometimes much in children; the average radiation from covered parts is less in women than in men. Extensive tables of the amount of radiation from equal surfaces of different parts of the body are given. A moderately cold or warm bath increases radiation afterward, as well as after exercise or friction. After the inward use of antipyretics, radiation increases as the bodily temperature sinks. The author believes the cause for increase and decrease of radiation is to be sought in a change of the physical and chemical constitution of tissues which is under the control of the nervous system.

Einfluss des Nervensystems auf die thierische Temperatur. Von Dr. Ugolino Mosso. Virchow's Archiv, October, 1886.

This prize thesis, by a young brother of the well known physiologist of Turin, is a résumé of a more extended paper published in Italian. The valuable work of Heidenhain in 1884 presents the history of the conclusion now so fundamental in physiology, that muscle contraction develops heat. It is only bad batteries, however, that do so. If it could be shown that heat continues to be developed after the muscle has ceased to contract, that the increase of heat is not proportional to the work, that by the constant contraction of a muscle the temperature of an organism cannot be increased, and that the temperature of the body may diminish while the muscular work remains the same, then it may be inferred that heat production is an attendant but not necessary phenomenon of contraction. The first of the above statements was proven by stimulating reflex frogs, from the thighs of which calorimetric readings were taken. Dogs were allowed to run inside wheels six metres in circumference for six hours, and rectal measurements of temperature showed, after a rapid rise for the first hour, a gradual sinking for the following five hours, reaching the intermediate point between extremes of temperature at the beginning and end of the first hour, and at rest sinking rapidly below the former. The temperature of Dr. Mosso's body during a two days' march was not in relation to the work done. Again, strychnine increases the temperature of the animal body, even after it has fallen through the influence of curara, and in spite of the most complete immobility of the muscles. In dogs the rectal increase thus obtained is as much as three degrees. Of the three places, brain, sinus, and rectum, where measurements were taken, which were about alike, the sinus temperature always decreased with muscle work. In experimenting with drugs causing convulsions, temperature always increased before cramps, and the blood temperature in the right sinus often fell during cramps. In curarized animals a rapid and lasting elevation of temperature was observed as a result of the infliction of pain.

Similar elevation of temperature was observed in man as a result of pain, but here the conditions were more complicated. The rectal temperature of dogs, which is very susceptible of variation, rose sharply at sound of a gun, and still more from the emotions connected with bringing them from the cool cellar, where they spent the night, into the laboratory, and also on seeing other dogs. Emotion also increased the temperature of pigeons. A strong emotion of joy caused in the author an increase of temperature amounting to nearly a degree, which had only sunk to half a degree four hours later.

Four Cerebral Heat-Centres. By ISAAC OTT, M. D., and WILLIAM S. CARTER. Therapeutic Gazette, Sept. 15, 1887.

In previously published results Dr. Ott claims to have shown that fever is mainly a disease of the nervous centres; that albumoses, peptones, the leucomaine neurine, produce fever through the nervous system; that antipyretics produce fever by acting on it, and that the ascription of fever, sleep, and the action of peripheral irritants to modifications of circulation is entirely erroneous. In this article he attempts to define more minutely the heat centres which he claims to have been the first to discover about the corpus striatum. The method was calorimetric observations on trephined rabbits. Four centres are found: 1, in front of and beneath the corpus striatum; 2, on the median side of the nodus curiosius; 3, the parts about Schiff's crying centre; 4, the anterior inner end of the optic thalamus. The last causes the highest rise of temperature, but the elevation caused by 2 and 3 lasts longer, sometimes more than three days. These centres have excitory and inhibitory power. Respiratory and circulatory changes attending puncture have no thermal effect. Puncture may either remove their inhibition on the spinal thermogenic centres, or cause them to act with these as exciting centres in exciting increased chemical metamorphosis of tissue. In an earlier article (*Journal of Nervous and Mental Diseases*, July, 1887) Dr. Ott claims to have shown that the thermo-inhibitory fibres decussate at the nib of the calamus, and in still another, this indefatigable experimenter (all in his private laboratory at Easton, Pa.) has explored the relation of the thermogenetic apparatus to atropine (*Therapeutic Gazette*, August, 1887).

Reactionszeiten der Temperatur-Empfindungen. Von GOLDSCHEIDER. Berlin. Physiolog. Gesellschaft, June, 1887.

A suspended metallic ball was so hung that displacement of it involved the breaking of an electric circuit for chronological measurement. The stimulus was made with closed eyes and by active motions of the person stimulated, and upon many different dermal points. The chief results were that temperature sensations come to consciousness later than those of contact, that cold is perceived much sooner than heat (15° C. and 50° C.), and that this difference increased with the distance from the brain, till it reached the relatively enormous amount of about half a second. With feebler degrees of thermal stimulation both the average and personal errors increased, as did the time. Still greater retardation of sensation from heat has been observed (Stern–Oppenheim) in tabes. Goldscheider does not think this difference between warm and cold due to different centripetal paths nor to difference in peripheral stimulation. The cause is not yet apparent.

Zur Physiologie des Geschlechtsapparates des Frosches. Von Prof. J. K. TARCHANOFF. Pflüger's Archiv, 1887, pp. 320–351.

Spallanzani and Goltz had found that the sexual embrace lasted from four to ten days till the last egg had appeared and been fructified, and that not only the strong fore legs and thumbs of the male, which were so firmly locked together that they could not be parted without lesion, but the whole nervous and muscular apparatus of embrace was in a state of strong and constant tonic excitation. Decapitation nor burning did not interrupt, nor burning nor abscission of limbs of the male prevent a renewal of the act. To answer the question, what is the impulse that proceeds from the female and what is the seat of excitation in the male, Goltz removed the ovaries, cord and brain and skin along the back of the female without lessening the ardor of the embrace by the male, which, however, refused a male sewed into the skin of a female, and concluded that every part of the female had a certain attraction. The various senses of the male were successively eliminated, and the conclusion reached that the attraction affected its every sensory apparatus. The reflex mechanism of the embrace was found to be located in the upper part of the cord, and to be excited from the skin between the fore legs, and after decapitation the finger of a man is clasped as tightly as the female with intact brain, but if this skin is removed the embrace no longer takes place. Castration did not affect the passion of the male nor even relax his embrace, but spots were found on the skin where the application of acids relaxed the embrace of the reflex frog. After repeating and confirming these experiments, Tarchanoff cut out the various internal organs of the male, including testes, one after another during the embrace without relaxing it. Only the emptying or excision of the seminal vesicle caused voluntary relaxation and lasting sexual indifference of the male. The same result followed section of the nerves connecting these vesicles with the central nervous system. Relaxation of the embrace by inhibition caused by painful reagents is far easier near its beginning than near its end, and with intact than with excised brain. Stimuli of the thalami or anterior portions of the corpora bigemina are especially effective in relaxing the embrace. This inhibition, the author inclines to think, is direct.

Ein gekreuzter Reflex beim Frosche. Von O. LANGENDORFF. Arch. f. Anat. u. Physiol., 1887, p. 141.

If a frog is held in the hand so that its hind legs hang down loosely, and the skin near the eye or tympanum be stroked with a blunt instrument, the leg on the opposite side is strongly flexed and abducted, and the web between the toes unfolded. The movement is tetanic, and continues some time after the stimulus is removed. This reflex, not provided for by Pflüger's laws, succeeds on nearly every frog, and even if the hemispheres and mid-brain are removed, but is inhibited by strong sensory stimulation on the same side. With electrical stimulation, when the kathode is applied to one and the anode to the other side of the head, the experiment succeeds also, but best of all with contra-lateral tetanizing induction currents. The crossing must take place beneath the medulla, but in what region of the cord it is not determined.

Myographische Versuche am lebenden Menschen. Von A. Fick. Arch. f. d. Ges. Physiol., September, 1887.

Change of tension in muscles so disposed that their length cannot vary is called isomeric contraction, and an apparatus devised by Fick to show this change of tension is called a tension indicator. The extended hand was laid in a simple frame with the palm or surface vertical, the thumb directed upwards with its ball resting against a solid wooden surface, and the index finger, to the second joint of which the indicator was attached, in a horizontal direction. The changes of length were of course not absolutely excluded, but were registered, greatly magnified, by a very long lever. Electrical stimulus was applied to the abductor indicis. Thus it was possible to reckon what Weber called the "absolute muscular power," or the direct pull of the muscle on that part of the bone leverage to which it is attached, which is a magnitude of the same order as that which Koster by another method determined for the muscles of the calf of the leg. No endurable degree and no frequency of electric stimulus can excite the same degree of tension as the will, but only at most about two thirds as much. A tetanus can develop from six to ten times as great tension as a single shock. While a frog's muscle of about the same size develops great energy of contraction from a single shock, a series of tetanizing shocks can hardly develop double the energy of one. The voluntary and electrical stimulus summate, but the greater the voluntary tension the less is the additional tension caused by electricity. The interval of time between the individual shocks is within wide limits indifferent. Besides this increased tension the electric stimulus there is a later reflex diminution of the voluntary tension. If the latter was maximal, only the reflex effect is seen on the indicator.

Ueber Ataxie und Muskelsinn. Goldscheider. Verhandlungen der Physiol. Gesellsch., Berlin, August, 1887.

Dr. Goldscheider reports experiments which favor the Leyden theory of spinal ataxia (which ascribes it to lesions of sensory tracts), as opposed to the theory of Erb and Friedreich (which ascribes it to centrifugally conducting co-ordination fibres). As both parties admit that in rare cases there may be extended and absolute anæsthesia produced without ataxia, as well as ataxia without disorders of sensibility, the question really focuses down to the problem of the muscle sense. To test this he rested the hand in a plaster mould, palm upward, and bent the index finger back by changing pressure of a small weight, measuring carefully the least angular bending at the first joint which could be perceived. A faradic current was then applied over the joint which caused nearly complete anæsthesia, when it was found that the finger joint must be bent far more to be perceived than before. Thus centripetal impressions from the nerves of the joint seem to be an element in the perception of passive movement. If active movements are attempted by a finger thus faradized, they can no longer be made continuously, but are intermittent, as well as excessive and more rapid, in other words ataxic, while the subject believes the movements to be uniform. With strong currents the graphic representation of both flexion and extension is like stairs. If the eye and attention are turned to the finger, the amplitude and rapidity of motion are reduced to the normal, but the intermittence can be but slightly

reduced. This phenomenon is ascribed to a reduction of sensation for changes of position. The greatly increased threshold value of the stimulus of co-ordinating the action of antagonistic muscles is the closing explanatory suggestion.

Ueber Unterscheidungszeiten. J. v. KRIES. Vierteljahrschrift f. Wiss. Philos., January, 1887.

According to Wundt, perception is the entrance of a conception into the inner field of vision, and apperception is its entrance into the inner point of vision; and he ascribes a distinct element of a total reaction time to the interval between these two processes. v. Kries doubts the wisdom of thus introducing figurative expressions which are not immediately intelligible into the description of psychic processes, and thinks that this formulation of Wundt runs some risk of overlooking important things and confusing different ones. It inclines uncritical minds to think that each concept, a certain time after it enters the field of mental vision, passes on to the focus of attention, and that thus apperception time of *e. g.* a complex object is always a quite definite time. v. Kries therefore prefers the term differentiation time, first used by him in 1877, because the different qualities of the same object are known in quite different times depending on the direction of attention, etc. In these earlier experiments the task for the experimenter was to give all his attention to determining whether a signal had a certain quality (*e. g.* was red or not), which is quite different from recognizing which of several colors appeared. Again, the so-called *c*-method of Donders requires simply reaction on *a* and not to *b*, and is not to be confounded, as Wundt does, with a choice between motion and rest. v. Kries's experiments involve only mental differentiation, and his results, such as that localization is quicker than judgment of intensity, optic direction than distance, and acoustic localization time increases with decrease of the angle of divergence, are not to be brought under Wundt's rubrics. Wundt's method of reacting after the judgment is made that perception has taken place, introduces an element of introspection which is too variable to give precise results. Differentiation time proper is here at least increased by a value of unknown magnitude, and it is impossible to exclude cases in which the impulse to reaction precedes knowledge. In such a series of psychic processes it is impossible to bring the reaction always at one and the same stage of each series, as much so as it would be to react at either the optic or the acoustic sensation of an electric spark at will. Results by Wundt's method are therefore doubted. Either the reaction is too quick, or else reflection time is added. Of Wundt's pupils, all have found, therefore, too long reaction times, and one of them, Cattell, even intimates that v. Kries not only often reacted prematurely, but often suppressed results, in one series in fact more than half of all. This v. Kries indignantly denies, and repeated his former experiments only to find them correct.

Kritisches und Experimentelles über den Zeitsinn. Von RICHARD GLASS. Philosophische Studien, IV, Heft 3, pp. 423–457.

The fact that in the sphere of the time sense, more than elsewhere, the conclusions of different observers stand in glaring contradiction to one another, induced the author to attempt to add his contribution to the topic. He follows in the footsteps of Estel and

Mehner, and used the same instrument, the essential parts of which consist in a device for marking off the standard time by the interval between two electric clicks, and for measuring the time between the last of these clicks and the stoppage of the apparatus, the latter being the time that the subject regards as equal to the first interval. Vierordt had found that small intervals are over-estimated, large ones under-estimated, an indifference point (where the estimate is correct) intervening. Estel found that at the multiples of the time at which this indifference point occurred there were likewise maxima of accuracy of judgment; he also concluded that Weber's law was not applicable to the time sense. Mehner's results are, that these maxima of accuracy occur only at the *odd* multiples of the indifference time (.71 sec.), the minima of accuracy occurring at the even multiples, up to about 11.4 sec. Furthermore, that intervals up to .7 sec. were exaggerated; intervals from .7 sec. to 5 sec. were under-estimated, and larger intervals again exaggerated. The author subjects these results to a rigorous criticism, the outcome of which is that a harmony in the results can be brought about only by not working the results for more than they are worth, and by taking into account the method, the unavoidable individual differences, and the rough and unusual sense exercise that is employed.

His own results are as follows: The standard times in the first series were the multiples of .7 (in the main) up to 15 sec.; and 100 observations on each time were made. He finds that points of greatest accuracy are at 2.8, 7.8, 9.3, 12 and 14.2 secs., and of minimum accuracy at 5, 8.5, 10, 12.8 and 15 sec. This does not agree with the periodicity of Mehner, but shows two groups of relative indifference points, each rising by an interval of about 5 sec. (2.8, 7.8, 12), (9.3, 14.2). The result of the second and more extended series is that the difference between the points of greatest accuracy is quite regularly 1.25 sec., with the exception that at .8 sec. there is a point not thus included. The law for the points of least accuracy cannot be traced. If we take into account that all the judgments are too long because they include parts of a reaction time and deduct $\frac{1}{10}$ of a sec. on this score, all the intervals (excepting that at .8) are under-estimated. The general conclusion supports Vierordt and opposes Mehner. Regarding Weber's law the author concludes that while decided deviations from this law occur (some of which can be explained), yet there is a strong tendency to follow the law as closely as the nature of the experiments would lead one to expect.

It will be seen that while this paper forms a real contribution to our knowledge of the time sense, it by no means places this topic in the clear light in which it should stand to gain recognition as a branch of accurate science. J. J.

Beiträge zur Theorie der sinnlichen Aufmerksamkeit und der activen Apperception. Von N. Lange. Philos. Studien, Bd. 4, Heft 7.

Attention strengthens sensations, so that even very weak ones may eclipse in consciousness those objectively far stronger. But for this specific power, present sensations would expel concepts, memory, etc., because the former are more intense. Attention, however, is no extraneous power. It is a name for the process of reinforcing one set of impressions by another set. In attention the will does not work directly on concepts. The will must not be divided into motive and

apperceptive will, and it cannot inhibit concepts. This whole problem has grown in importance with the decline of the English theory of association, and the latter is due to the neglect of the phenomena of active apperception. Very feeble sensations strongly attended to alternately vanish and grow intense. The duration of the periodic wave of attention can thus be measured. Helmholtz had noticed this vacillation of unusual optical impressions in experiments with Masson's discs, and Urbantschitsch had noticed a similar phenomenon in the ticking of a watch not due to objective variation or to peripheral organs, but to central changes in attention. It is observed in cases of perforated tympanum, and so cannot be due to periodic tension of entotic muscles. That this is not due to the fatigue of the acoustic nerve, as Urbantschitsch thought, is shown by the fact that when the phenomenon is observed for both optical and acoustic sensations simultaneously, the two periodicities of the two series of sensations do not coincide, but are separated by a fixed interval. Thus the cause cannot lie in independent peripheral organs, but must lie in a common centre. Lange was able to register these vacillations of intensity chronoscopically, not only for one, but for two kinds of sensation simultaneously. These periods are longest for sound (3.5 to 4 seconds), next longest for light (3 to 3.4 seconds), and shortest for faint electrical stimuli (2.5 to 2.6 seconds). The average variation was less than one fourth of the entire period.

De la Répartition du Sang circulant dans l'Encéphale. Expériences faites au laboratoire de physiologie de l'Université de Bruxelles. E. SPEHL. L'Encéphale, 1887, Vol. I.

The old theory that the brain was congested in sleep was first effectively combatted by Durham in 1860. Since then the anaemic state of the cortex has been experimentally proven in four ways: 1. By experimentation on animals by Claude Bernard, Weir Mitchell, and others; 2. A little later, by observations of the movements of contraction and expansion in patients who have lost a part of the skull; 3. By observations on the same class of patients by the more precise graphic method—last and chiefest by Mosso; 4. A method preferred by Hammond, of ophthalmoscopic observation of the retina as reflecting the vascular state of the brain. The method of Spehl was to apply about the neck of a rabbit an apparatus by which all connection between the head and trunk could be instantly arrested and decapitation be then carefully made. Five animals in the normal condition were subjected to this treatment, and the weight of the whole body and that of the quantity of blood in the head and trunk carefully determined. Five more were treated then in the same way in a state of sleep induced by chloral, and the results compared. The average proportion of blood in the head in the latter series had sunk from one eighth to more than one eleventh, confirming thus the general conclusion that in sleep the brain as a whole is anaemic. The mode of experiment does not of course admit of discrimination between the quantity of blood in the head and in the brain only, and the inference from sleep produced by normal sleep to the hypnotism of chloral is obviously only highly probable. The author suggests, in conclusion, that the differences of opinion that have prevailed may be due to the active parts of the brain being congested and inactive parts anaemic at the same time.

II.—HISTOLOGY OF THE NERVOUS SYSTEM.

Ueber den Ursprung und den centralen Verlauf des Nervus Accessorius Willisii. Von Otto Dees, München. Allg. Ztschr. f. Psych., XLIII, 4.

The author studied the origin and central course of the accessory nerve in the normal human cord and in the cords of rabbits in which the nerve had been removed by v. Gudden's method. At the level of the olivary body the accessory fibres cease. Below this they first arise, in the oblongata, from cells in the middle of the anterior horn (superior nucleus). This group of cells becomes more lateral, and between the second and fourth cervical nerves lies on the lateral edge of the anterior horn (the median nucleus), while below this it occupies the base of the lateral horn (inferior nucleus). Certain fibres emerge at the level at which they arise, others run towards the head, then turn at right angles and come to the periphery.

Sulla degenerazioni discendenti consecutive a lesioni sperimentali in diversa zone della corteccia cerebrale. Dei Dot. V. Marchi e G. Algeri. Revista speriment. di freniatria e di medicina leg., 1887, XII, p. 208.

The authors experimented on dogs and monkeys, from which they removed parts of the cortex, and then, having allowed the animals to live months or years, searched the oblongata and cord for degenerations. (1) On three dogs a piece of cortex 1.5 cm. on a side was cut away from the motor region (D. H. C. of Munk's figures). (2) On three more dogs a piece of the same size in the intermediate region (F. of Munk) was removed, and in (3) a final three a portion of the occipital region (A. of Munk). (4) Finally a monkey had the region A on both sides removed, and square piece in the upper third of the central convolutions on both sides. In all series there were motor disturbances—most marked in the first and least so in the last. Sensory disturbances in every case also. These were least marked in the first and most so in the last. The disturbances did not in either case entirely disappear during the time that the animals were allowed to live.

The degenerations found were distributed among motor and sensory tracts in the cord in a way roughly indicated by the reactions of the animals. In 1, degenerations were found in the crossed and uncrossed pyramidal tracts and some fibres in the columns of Burdach. In 2, partial degeneration of the crossed pyramidal tracts, extensive degeneration in both columns of Burdach, and scattered atrophic fibres throughout the entire section of the cord. In 3, the degenerations were as follows: Complete atrophy of the crossed column of Burdach, while the crossed column of Gall and the entire uncrossed posterior column showed only a few degenerated fibres. In 4, the monkey, which was kept alive two years, the entire extent of both post. columns was degenerated, and there were scattered atrophic fibres through all the other parts. It is concluded from these results that sensory and motor fields to a certain extent coincide in the cortex, and that a complete crossing of sensory or motor fibres is not to be assumed.

Zwei Feuerländer Gehirne. Von Dr. Joh. Seitz, in Zürich. Zeitschrift für Ethnologie, 1886, Heft 6.

One of these Fuegian brains was that of a man, the other of a woman, the respective capacities being 1710 cm^3. and 1370 cm^3. This gives an estimated brain weight of 1631 gr. and 1370 gr. The author concludes, after careful study of them, that "The weight is average and the measurements average. The measurements of the fissure of Rolando are like the European. As regards convolutions and fissures of the cerebrum, the representations of European brains are in all respects applicable to these brains of savages." The author calls attention to the fact that other investigators in this line reaching other conclusions, have often described variations from the ordinary as marks of a low type.

Ueber das Riechcentrum. Eine vergleichend-anatomische Studie. Von Prof. Dr. E. Zuckerhandl, in Graz. Stuttgart, 1887.

From a careful comparative study of the callosal convolution (Balkenwindung), first described by the author, and its associated parts, Z. describes the following as the anatomical basis for the sense of smell: 1. Cortical portion: Ventral portion and frontal end of the lob. corp. callosi, lob. hippocampi with the uncus, Ammon's horn with the marginal convolution, cortex of the peduncul. olfactor., of the lam. perforat. anter., and the bulbus olfactorius. 2. Radial fibres: Inner marginal convolution. 3. The union of identical regions in the two hemispheres is effected through the ant. commissure. 4. Association paths: The fibrae propriae of the convolutions named—the forceps and a part of the fornix and alveus.

On the Histology and Function of the Mammalian Superior Cervical Ganglion. By W. Hale White. Journ. of Physiology, 1887, Vol. VIII, No. 2.

To his previous investigations the author has added the study of 41 sup. cerv. ganglia from human adults, 10 from human foetuses, and 46 from the higher mammalia. The results are: 1. In man the ganglion is very variable in size, while in animals it bears a direct relation to the size of the creature. 2. In man there are proportionately more atrophic cells with granular pigment than in other mammalia—monkeys are most similar to man—but these cells disappear as one descends in the animal series. 3. The ganglia in the human foetus show only normal cells. The author concludes that in the adult we have to deal with a stunted organ, and further investigation furnishes grounds for the view that what is true of the sup. cerv. ganglion is true for the entire sympathetic nerve.

Ueber die Bedeutung der Hirnfurchung. Von J. Seitz, Zürich. Jahrbücher für Psychiatrie, 1887, Bd. VII, Heft 3.

The author looks on the form of the convolutions as something to be explained in the same way that the external form of the species of which they are characteristic is explained. The fissures and furrows are mechanical aids to nutrition. The topography of the brain is influenced by all the causes which influence growth, and the true significance of the convoluting of the surface can only be understood when all these factors are considered.

Sulla fina struttura dei corpi striati e dei talami ottici. Del Dott. V. MARCHI. Revista speriment. di Freniatr. ecc., 1887, XII, p. 285.

The author here presents the results of several years' investigation on the structure of the corpora striata and the optic thalami. The entire investigation is based on Golgi's work. He finds the cells irregularly scattered through both ganglia. Those of Golgi's first type, or the so-called motor, are most abundant in the optic thalami, while those of the second type, or the sensory, are most abundant in the corpora striata. The fibres enter the cells of the first type only. It follows, therefore, that he considers the optic thalami as motor in function, and the corpora striata as sensory.

Ueber den Kernursprung des Augen-Facialis. Von E. MENDEL. Neurologisches Centralblatt, 1887, No. 23.

The author points out that in 90 per cent of the cases of apoplexia sanguinea the mouth-facialis is affected while the eye-facialis is not. That in bulbar paralysis the facial nucleus is found degenerated, and yet the eye-facialis is not affected. He removed in rabbits and guinea pigs, by modification of the method of v. Gudden, the muscles supplied by the eye-facialis on one side. As a result, the posterior part of the oculo-motor nucleus on the same side was found atrophic. The fibres from these cells to the facialis stem run apparently through the posterior longitudinal bundle. The pathological evidence, so far as it exists, favors the location of the eye-facialis in the homologous nucleus in man. It is another example of the central concentration of the nuclei of associated muscles.

Ueber den Ursprung und den centralen Verlauf des Acusticus. Von v. MONAKOW. Correspondenzbl. f. Schweizer Aertzte, 1887, No. 5.

The author made use of v. Gudden's method on cats. As a result of these experiments the probable track of the acusticus fibres from the periphery to the cortex is given as follows: Posterior root, superficial layers of the tuberculum acusticum, striae arcuatae acusticae, fibrae arcuatae crossing in the raphe, dorsal medullary substance of the superior olive, the inferior lemniscus, corpora geniculata interna, posterior bigemina and their arm, temporo-occipital lobe.

H. H. D.

III.—ABNORMAL PSYCHOLOGY.

(1) *Der Traum als Naturnothwendigkeit erklärt.* Von W. ROBERT. Zweite Auflage. Hamburg, 1886. 53 pp.

(2) *Das Leben im Traum.* Eine Studie, von Dr. PAUL SCHWARTZKOPFF. Leipzig, 1887. 102 pp.

(3) *Schlaf und Traum.* Eine populär wissenschaftliche Darstellung, von Dr. FRIEDRICH SCHOLZ. Leipzig, 1887. 70 pp.

(1) Different students, such as Strümpel and Hildebrandt, have noted that the materials of which dream images are made have come either by suggestion of trivial experiences of recent waking life, or are such stimuli incorporated, with little or much modification,

into the dream drama. Robert seeks to make this fact the key to all dreaming, and to explain the phenomena as a necessity for rational psychic life. Whenever we receive an impression we tend to act upon it, to elaborate it as it were, and to appropriate the useful by storing it away in memory. We receive many impressions, some of them of interest, but which we have not time to attend to while we are busied with the daily duties. At night these impressions, thus temporarily set aside, come up, pressing their claims upon us when we have leisure to attend to them. Most of these impressions, together with refuse of ideas digested during the day that lies like chips in our mental workshop, are swept out, chiefly during the early part of sleep. But morning dreams are more elaborate. These work up undigested material which is of use to the psychic economy. In this way does the brain solve our problems for us during sleep. Insanity is simply an overwhelming flood of unarranged ideas, and hence the value of sleep as a restorative to sanity. Robert thinks all attempts to classify dreams are futile. He illustrates his theory by relating and explaining various dreams; also notes dreaming of disease and of drugs.

(2) Our second author emphasizes the idea that we have psychic activity in sleep to such an extent as to make dreams the real life of the soul. In dreams we lose nothing of our character, not one jot of mental power. The flighty, illogical, magical, disconnected and incomplete nature of dreams is due to the fact that a stable world with its continuous stimuli has been shut out from us by the closure of our senses, and the stimuli we do receive come at intervals and in a sudden way, startling us and exciting our emotional nature. The mind, as Lotze says, of its own power creates images when the sign of the stimulus presents itself, and this is in fact the sensation itself. In dreams the mind likewise can create sensations just as real as in waking life, and that whether there be external stimuli or no. Even in waking life we have power to withdraw our attention from external stimuli and in abstraction live. A sensation, a perception or apperception, a representation (memory, fancy, imagination), an hallucination, all are one simple act of mind, but differing in intensity and concomitants; and all these modes we can exercise whether awake or asleep. In waking life we do not recall dreams to any great extent, but likewise in sleep we have forgotten the objective world and our past experiences. All the different gradations of activity felt in dreaming may be experienced while awake. It is all a mental creation like unto that exercised by poets and dramatists. The laws of association obtain in a similar manner in both states of life. Dreaming is a purer activity of mind, and shows its best fruits in the visions of seers and prophets.

(3) Dr. Scholz has given us a very readable brochure covering the entire subject of sleep and dreaming, with a third section on sleeplessness, its causes, and a general consideration of the hygiene of sleep. The standpoint is that of modern physiology; the figure, that of ebb and flow of the tide. In sleep the cerebral activity is at a minimum. Pflüger's theory of sleep, elaborated by Preyer, is adopted as the best theory, though it must be confessed that it does not offer a complete explanation. All activity of protoplasm is accompanied by oxidation—the breaking down of complex molecules that have previously been built up in connection with absorption of oxygen. This absorbed or intramolecular oxygen is, as it were, a granary that furnishes force for the activity of the cell. But when

the cell is active it uses up oxygen faster than it receives it (katabolism overbalances anabolism), hence must come a period of rest from activity where anabolism has the upper hand. During the katabolic process the products of decomposition gather in the cell and clog its activities. Then the cell desires sleep. When the refuse has been cleared away and the waste made good by synthetic processes, the cell once more is ready for work, its protoplasm is irritable and explodes at the first stimulus, the cell awakes. When we transfer this picture to the cells constituting the higher psychic ganglia, and hold that mental activity is accompanied by and dependent on cerebral cell activity, we have a good explanation of the cause and phenomena of sleep. The author then explains the laws of sleep by application of the above hypothesis.

In considering dreams, Radestock is principally followed. The controlling centre being asleep, the lower centres are free to act as they have a chance, and are stimulated to activity either by impressions of the senses, or from the abnormal processes in the body, or lastly, by the spontaneous play of the least tired cerebral cells themselves, recalling memories that have not been in the mind for some time. Much comes from the vast region of the unconscious. In the elaboration of dream images, the laws of association will of course find free play. The attention not being fixed is freer to respond to faint impressions, hence incipient disease in the body is able to arouse prophetic dreams.

No attempt can be made to give a complete abstract of a paper like this, which is itself an abstract well worth reading. J. NELSON.

Zur Pathologie des Gedächtnisses. Von Dr. A. PICK. Arch. f. Psychiatrie, 1886, p. 83.

While partial anmesia, especially in the field of speech, has been carefully studied of late, comparatively little has been added during the last ten years to our knowledge of general anmesia, and the cases that have been studied with detail are mostly progressive and not regressive cases. A married woman of 27 as a sequel of peritonitis quite lost memory of her own name and age, marriage, child, etc. When she came to the asylum at Dobzean, of which the author is the director, she was unable to remember whether she had taken her meals, the day of the week, the year, whether she had ever seen the doctor, etc. Gradually, however, the details of her past life were recalled, recent and remote events revived, and at the end of three months her memory seemed quite normal. Optical memory images seemed most completely extinguished, so that the case may illustrate asymbolism in the changed sense that Wernicke gave to the term as originally suggested by Finkelnburg. In the case of patients who confuse persons and objects, get lost in well known streets, chew coal, exchange ingredients in cooking, loss of memory images is probably the real cause of what is often diagnosed as delusions. In this case the tests by questions, pictures, etc., were very numerous, and the law that the most familiar concepts of daily life were first regained was strikingly illustrated, thus affording the often desiderated complement to the frequent observation that these concepts were the last to be lost in regenerative cases. Koempfen's law that loss of memory proceeds backward in time from the trauma toward childhood, and return of memory is from the remoter past to the present, is also in general well confirmed in this case. But

during the period of convalescence the patient was strongly prone to localize events in time according to their vividness in her memory, indicating weakness of associative processes. The high degree of apathy shown by the patient in the lower stages of her mental obnubilation seems to have been due in part to the general exhaustion which weakened memory also, and also in part to the loss of the stimulus that a regular supply of memory images furnishes. This patient had so much better memory for auditory than for visual images in youth that the period of their return was separated by a marked interval. Loss of memory is so commonly associated with unrecoverable cases, or the restoration of memory, if it occurs, is too sudden to afford opportunity to study its stages, so that altogether this must be called a noteworthy case.

Illustrations of Unconscious Memory in Disease, including a Theory of Alteratives. By CHARLES CREIGHTON, M. D. London, 1886.

This book is a remarkable illustration of interpreting the physiological by the psychological, rather than the reverse, as is more often done, and seems to have been suggested by Hering's lecture on "Memory as a Function of Organized Matter," and by Hartmann's "Unconscious." Consciousness on the one hand and generation on the other represent the extremes of explicit and of implicit memory. Generation is potential, consciousness actual, memory. Every lapse from or retreat behind consciousness represents the tendency to involution toward the above acme of implicitness. Repairs and growth, especially of new tissue after traumatis, are a reminiscence of embryonic activity. The memory of development is concentrated in the ovaries, and ovarian tumors are fantastic and perverted productions. Reproduction is the deepest rooted memory. All diseases, in fact all reminiscences, perverted or not, is of earlier states or experiences of the individual or ancestral organism. A neurotic person, *e. g.*, has a retentive memory. Alteratives are means of habit-breaking. Does not the instinctive doubt which arises as to the soundness of Dr. Creighton's method imply a deep-seated distrust in the normative nature of consciousness?

Remarkable Case of Sudden Loss of Memory. F. P. DAVIES, M. D. (England). Am. Journal of Insanity, April, 1887.

A young man of 22 was brought to an English asylum in July, 1886, who had apparently lost all memory of his own name, friends, or past life. He habitually wore a puzzled look, and spent much time in trying to recall his past life. After a few days he began to have "inspiration." The name of a person he knew came back or was "revealed" to him, and later another; but both these persons when written to denied all knowledge of the man described. He became depressed, and wrote intelligent letters indicating much mental culture, to others, describing himself, but compelled to subscribe himself as "Unknown." His photograph was taken and sent in vain. After about four months it was half believed that he was malingering, and he was put into an unpleasant ward and told he should not leave it till he had ended his game and told his name and address. The next day these came back to him and he wrote letters to his friends and was taken away. His memory now returned rather rapidly. The author was convinced that the loss of memory was genuine, and that it came on during two days of helpless

wandering in an abnormal state in which he was first found by the police. He proved to have been a clerk of ability in a large establishment which he had suddenly left because a demand for more salary was refused, and had disappeared. Was it epilepsy?

Habit in Insanity. By A. B. RICHARDSON, M. D. Am. Journal of Insanity, April, 1887.

The three elements determining habitual discharge of nervous energy are (*a*) congenital disposition; (*b*) experience under external conditions; (*c*) inhibitory and directory power over the will. The latter two can be to a degree controlled. Disease especially, however, weakens the will, and the insane are more imitative than the sane. In the early stages of disease bad habits can be best modified for the better. Even delusions which are often very fixed may be removed or replaced by others less incurable, not by argument, but by environment and treatment. Fixed habits of treatment by physicians are responsible for many bad habits of patients. Habits of taking certain drugs, habits of filth and untidiness, laziness, destructiveness, and even homicidal propensities, may be greatly modified for the better by persevering adaptation of treatment to individual cases, remembering that "our nervous system grows to the modes in which it is exercised." Love of this adaptation and individual study is the best guarantee that a physician is growing in excellence. The writer has tried his method with great success in his own asylum. The patient must be always placed in a position most favorable to reassume sane habits.

Lecture on the Disorders of Language. By Professor BIANCHI, Naples. Alienist and Neurologist, April, 1887.

This article, translated by Dr. Joseph Workman, of Toronto, the well known and still vigorous octogenarian alienist, represents that the four elementary factors of speech (two sensory—hearing and seeing—and two motor—speaking and writing) which have been developed and inter-related in the evolution of speech, connect with functional extrinsication of diverse parts of the brain; writing and reading being of course much later ontogenetically and philogenetically than hearing and speaking. Hearing is located on the first temporal convolution and a part of the second; seeing words in the inferior parietal lobule; speaking in the foot of the left inferior frontal convolution, and writing at the foot of the left second frontal convolution. Each centre is situated within larger related areas, the motor in the wider field of arm or tongue and jaw motions, and the sensory are specialized centres within the field of hearing and sight; and these special as well as the wider general centres are very closely related, so that a disease of one without an affection of others is rare. Each centre, too, is the focus of memory images. Thus one may hear but not know the meaning of words, as is the case with a child. This is the sensory aphasia of Wernicke, or the verbal deafness of Kussmaul, and is often associated with paraphasia. Lichtheim's view that the inner acoustic image, or internal diction, is indispensable to correct pronunciation is refuted by clinical facts. His theory that simple verbal deafness is due to lesion of the centripetal auditive paths before their entrance into the centre, it being sound, is opposed to that of Charcot and Kussmaul, that this may be due to lesion in the acoustic centre, while speaking, reading

and writing may be possible. In cases of pure verbal deafness the acoustic may be represented by the visual image. Bianchi dissents from Charcot's view that verbal deafness and verbal amnesia may have the same seat, but in the first case be destroyed and in the latter only superficially injured, and holds that amnesia may be due to interruption between the idea and acoustic centre, or due to enfeeblement of ideation. Verbal blindness is much less apt to be associated with psychic lesion than verbal deafness, although the latter often co-exists with intact ideative processes. If DeWatteville's view be correct that reading is possible only with true mnemonic and reproduction of acoustic images, then verbal blindness ought to be caused by interruption between the acoustic and visive centre. An interesting case of an aphasic is given who could not get the name "hat" from seeing it or even touching it, but just attained the word by taking the hat and putting it on his head with some force with both hands, and could pronounce the word "key" only in the act of turning a key. Whether this is sensory (Wernicke) or motor aphasia (G. Stewart) it is hard to tell, but the *post mortem* finding did not indicate disease of the word-centre.

On different Kinds of Aphasia, with special Reference to their Classification and ultimate Pathology. By H. C. BASTIAN. pp. 28. 1887.

Fourteen forms of defect in speech and writing are distinguished. The basis is anatomical, and the terms commissural aphasia, glosso- and cheiro-kinaesthetic centre may serve to suggest the chief novelties in this vexed field of classification. The paper contains interesting and new cases and diagrams.

De la Guérison de la Paralysie Générale, et de la Théorie des Pseudo-folies Paralytiques. BAILLARGER. Annales Médico-psychologiques, 1887, No. 1.

General paralysis is so universally held to be incurable that whenever a case of recovery has been reported it is at once set down, obvious and unique as the symptoms are, as a case of error in diagnosis. Even the case of recovery so minutely reported by Tuczek (in his Beiträge zur pathologischen Anatomie und zur Pathologie der Dementia paralytica) has been called an error of this sort. Baillarger, however, here reports a case of a man of thirty-nine who became prodigal in his expenses, excited, sleepless, with delusions of greatness as to his wealth, the number of his children, thought himself Pope and Emperor of Germany, and lost power to articulate certain sounds. About a year after he entered the asylum (1878) he had begun to recover, and at the intercession of his friends was granted leave of absence, the certificate of release stating, however, that he was enjoying a remission, that a fresh attack was certain, and that his intelligence was already greatly enfeebled. He quite recovered, and in 1882 resumed his post of business. Vision, however, was gradually impaired in one eye, and in 1884 symptoms of locomotor ataxia were fully developed. The author objects to the distinction between true general paralysis characterized by chronic periencephalitis, and pseudo-general paralysis due to simple circulatory derangements, and prefers to say that two distinct maladies have been confused under the term of general paralysis: one being characterized by diffuse delirium of greatness, hesitation of speech, and being quite distinct from general paralysis, the early stages of which it resembles, and being, unlike it, curable.

Paralysis, Cerebral, Bulbar, and Spinal. H. C. BASTIAN. London, 1886, pp. 671.

Paralysis is the most pathological of nervous diseases, and the author even goes so far as to state dogmatically that "almost if not quite all paralyses are invariably caused by definite morbid conditions, appreciable by the naked eye or by the microscope, or by both," although this would hardly hold of paralyses of sense nor of functional paralysis of motion, to say the least, both of which are treated in this book. The writer can and does confine himself largely to a basis of nerval and morbid anatomy, but the cuts, which are quite numerous, are mostly old and familiar. The regional diagnosis, to which much space is devoted, is good, but on the whole is written from the standpoint of the physician rather than of the scientific physiologist. The strictly pathological diagnosis is treated best of all, and the many convenient tables constitute probably the chief value of the book. The chapter on disorders of intellectual expression by speech and writing covers but thirty pages, but contains a valuable table for the examination of aphasic and amnesic patients. The chapters on spinal paralyses, in which such remarkable advances have been recently made, are brief, but quite adequate to the needs of the practitioner, whose wants are throughout kept mainly in view. The book is a vast and thoroughly well ordered collection of material, and is, on the whole, even more valuable than the author's previous work on Brain Physiology.

Remarks on Evolution and Dissolution of the Nervous System. J. HUGHLINGS-JACKSON. Journal of Mental Science, April, 1887.

In this valuable article the well known views of the author are summarized and widened. In severe epilepsy crude activities in all parts of the body, and at once, are produced, such a discharging lesion beginning in the latest and highest level of evolution. The post-paroxysmal state is dissolutive, which may reach almost total dementia, which is persisting coma, and recovery from which is re-evolution. There are high, low, and mid-level fits, representing different evolutionary levels. Laryngismus stridulus, *e. g.*, is a low-level, bulbar fit. Even a small and local physiological fulminate, if sudden and rapid enough, may set up discharges in healthy nervous tissue associated collaterally downward, and end in severe convulsion. Among the different insanities, melancholia (posterior lobes?) and general paralysis (anterior lobes?) signify different local dissolutions of the highest centres, as surely as brachioplegia or cruroplegia signify dissolution of middle, or ophthalmoplegia, of the lowest motor centres. In post-epileptic insanity, mania is the outcome of activities on the levels of evolution remaining, and the union of high special action with great defects of consciousness in some of these cases is due to deep dissolution in one hemisphere co-existing with high evolution in the other. Alcoholism on the other hand produces uniform dissolution without the phenomena due to different levels. The level of evolution also varies in different centres, and is a co-factor with the depth of lesion. Positive symptoms, as *e. g.* illusion, are evolutionary on a reduced but then highest level of a nervous system mutilated by disease. The hierarchy of the nervous system, which is throughout a sensory-motor mechanism, is threefold. 1. The lowest level consists of anterior and posterior horns of the spinal cord, Clarke's visceral

column and Stelling's nucleus, and the homologues of these parts higher up. It represents all parts of the body most nearly directly. 2. The middle level consists of Ferrier's motor region with the ganglia of the corpus striatum and of his sensory region, and represents all parts of the body doubly indirectly. 3. The highest level consists of the prae-frontal and occipital lobes, or of the highest motor and sensory centres respectively, these being the organ of mind, and evolved from the middle as the middle are from the lower and the lower from the periphery, and this re-represents the body triply indirectly. The division between the middle and highest is less decided than between the middle and lowest; and the author confesses some doubt at present as to the occipital lobes being the highest sensory centres. These three levels represent progress from homogeneity to heterogeneity, and increasing degrees of differentiation, specialization, integration, and interconnection, which are the four chief factors of organic evolution. The term "indirect" means that in going up the levels there are not insensible gradations but occasional stoppages. The pyramidal tracts, *e. g.*, connect the lower with the middle centres, and thus raise them to much higher powers. Centres are both reservoirs and resistances, and it is these latter or protective activities that make the physical basis between faint and vivid states of consciousness. Trains of thought, or internal evolution, independent of present experience, can go on. High centres are most complex, least organized, most needing to be forced into activity, but more capable of new kinds of action. Inward and upward the sensory centres are overcome in order, and finally great irradiation in the highest sensory centres occurs, and a "survival of the fittest" states. Downward there is a narrowing of the energy of liberation, as the possible is made actual. *Les grand maux* differs from *les petits maux* in that in the former the middle centres offer less resistance. The highest centres are least automatic, most imperfectly reflex, and seats of most active evolution, and universally representative of the whole organism and all its processes. It is the physical basis of the ego which is co-ordination or representation, the two latter terms being taken as equivalent. To understand the brain we must not take "a too brutally materialistic view" of the mind. Between mental and cerebral states there is perfect concomitance, not identity. Darwinism does not imply materialism, and this "two clock theory" is "convenient in the study of nervous diseases." The author does not attempt to explain mental states, but the structure of the brain, and, which is greater, what parts of the body it represents, and how to identify the two, is a metaphysical way of making short work of a hard problem. The chief use of mental symptoms for the physician is as signs of what is going on or ought to in high centres. The conception of concomitance rejects such terms as emotional or volitional centres, ideo-motor physiology of mind as logical cross-divisions, and declines to say the mind influences the body, fright makes the heart beat, or that sensations, ideas, etc., produce movements. Such expressions imply disbelief in the doctrine of the conservation of energy. Movements always arise from liberation of energy in the outer world, and it would be strange if an immaterial will interfered with the activity of nervous arrangements of the highest centres. On the concomitance theory we should not say an act was not done from lack of will, an aphasic did not speak because he had lost the memory of words, or a comatose patient did not move because he was

unconscious, but should try to find materialistic explanations of physical inabilities. As to how far down in the higher nerve centres consciousness attends nervous action, whether we judge by elaborateness of action or by memory as necessary, it is impossible to tell by loss of consciousness just how far down the lesion has extended. But as evolution proceeds consciousness is raised higher, and in dissolution activities on lower levels may have attendant states of consciousness. A typical fit of epilepsy is analyzed to illustrate the author's view so far, a scheme of future work is presented, in which constant reference must be had to the evolution of higher from lower, *e. g.*, as follows: 1. Centres for simplest movements of limbs become evolved in the highest centres into the physical bases of volition. 2. Centres for simple reflex action of hands and eyes evolve into the physical bases of visual and tactile ideas. 3. Centres for tongue, palate and lips, as concerned in eating and swallowing, became bases of words as symbols of abstract reasoning. 4. Lower circulatory centres became the bases of emotions. Thus the highest evolved from the lower becomes independent of it, and is the emotional basis of mind. Three degrees of post-epileptic insanity correspond to three depths of exhaustion. Even after a very slight fit there is defect of consciousness as to present surroundings with increase of consciousness as to some earlier surroundings, thus occasioning what often seems like two different mental states. Progressive muscular atrophy, paralysis agitans, and general paralyses of the insane, are alike in being due to decay of cells in order of size from small to large, but unlike in occurring on the lowest, middle and highest levels respectively of motor evolution.

Some of the Relationships between Epilepsy and Insanity. By Dr. C. H. SAVAGE. Brain, January, 1887, pp. 446–56.

Under the treatment of Hughlings-Jackson, who, in his epoch-making work on epilepsy, took the first important step towards applying the philosophy of evolution as represented by Herbert Spencer to the psychology of mental diseases (cf. the remarkable way in which, before and after Griesinger, Herbartian conceptions dominated the field of morbid psychology in Germany), this disease has come to be of the utmost interest to psycho-physicists. Dr. Savage, whose little book on "Insanity and Allied Neuroses," gives evidence of unusual discrimination, breadth and independence, here suggests two classes of epileptics: first the neurotic, with inherited nervous instability, and second, the organic or accidental, due to definite lesions in the brain itself. He thinks "masked epilepsy" rarely occurs without being preceded by fits; of which violent, acute and repeated dreams, occurrences that cannot be accounted for, or gaps in life that cannot be quite filled by the patient, are ample evidence. He suggests that those who are epileptic by heredity may be able to bear more nervous disturbance than those of apparently good stock, and thinks that the study of chronic epileptics may be as useful to the philosopher as the weathering of the rocks to the geologist. Singular cases where epilepsy serves to restore mental balance, the outbreak of severe fits coincides with the cure of even chronic insanity, are given. It is suggested that there may also be some relation between hallucination of smell, so very common in epilepsy, and the prevalence of the same delusion in those who are insane with

sexual disorders. As to the rate and direction of destruction of the mind in epilepsy (which after all is only provisionally a "disease," rather than a group of symptoms which happen to occur together, often under very various conditions), Dr. Savage thinks that either memory may be chiefly affected, when dementia may supervene in extreme cases, or that loss of control, more liable in furious cases, may be caused, when mania may result. Severe fits at long intervals are less degenerative than slight ones following each other at frequent intervals, for the latter preclude the possibility of the accumulation of energy, drawing it off as fast as it is stored. The other relations, lightly touched on, between muscular and psychic automatism in the status epilepticus are of great interest to the psychologist.

Der Verlauf der Psychosen. Von R. ARNDT, Professor der Psychiatrie an der Universität Greifswald, und Dr. A. DOHM. Wien, 1887, pp. 48.

In Arndt's *Lehrbuch für Psychologie*, 1883, an attempt, too little noted, had been made to reduce psychoses to the more scientific laws of nervous excitation and muscle contraction as demonstrated in experimental physiology. This, it was said, brought forms of psychoses, commonly regarded as remote, near together, and gave a new and transparent basis of classification. Psychoses are no longer diseases, but symptoms, like pain, cramp, etc. Instead of regarding melancholy, *e. g.*, as a state of depression and mania as a state of exaltation, each is conceived as an hyperaesthesia or hyperthymia, occasioned in the former case by a depressed and in the latter case by an exalted ego. Melancholy is thus widely distinguished from stupor, with which it is often too closely associated in classification, and furor may be melancholic or maniacal. In place of the ever growing complexity of psychiatric classification, Arndt would group all forms of psychoses about the fundamental laws, that feeble stimuli arouse nervous activity, medium stimuli increase it, strong inhibit, and very strong destroy it. Fourteen colored tables, representing as many typical cases from the insane asylum at Greifswald, are presented graphically to illustrate the above principles according to a method first presented by Dohm, the other author, in an inaugural dissertation in 1885. In these ingenious tables an ideal or indifference line represents repose or normal poise. From this horizontal middle line, a curve representing departures from this state is either upward toward mania or downward toward melancholia. To a certain extent up or down these fluctuations of "ergasia" are still within the latitude or tropics of health. Departure beyond these limits either way has seven degrees up or down, expressed by moods or acts of increasing abnormality, and culminating both ways in unconscious acts explosive (as distinct from impulsive) in their character. The aesthesias or "modifications of feeling or self-consciousness," are divided into ten forms, and are represented by hatched and cross-hatched lines constituting the background of the curve. Finally, paraesthesias are on violet; hypochondrial cases on brown; hysterical on green; alcoholic on blue, and epileptic on red background. Color in the last four cases shows thus the constitutional anomaly on the basis of which the psychosis is unfolded. The significance of the hatching is thus explained. Every form of self-expression is reflex. If sensations are retarded and inhibited, as they are especially liable to be in hyperaesthesic

states, so that their reflex effects do not readily flow off in the motor, secretory or trophic regions, a sense of psychalgia or melancholia results; conversely, if the system is less sensitive, and stimuli pass to their reflex effects without inhibition or with loss tension, feelings of pleasure tending towards mania arise, and the consciousness of the ego is exalted.

Chorea und Psychose. Dr. SCHUCHARDT. Allg. Zeitschr. f. Psychiatrie, January, 1887.

After an extended and valuable sketch of the history of medical opinion concerning chorea, in which it is shown that the St. Vitus Dance of the Middle Ages, or chorea magna, is more nearly allied to hysteria than to true chorea, and that there is a widely extended opinion among writers upon chorea that it is closely allied with forms of psychic disturbance, Dr. Schuchardt describes six new cases, and concludes that the choreic type of psychosis is characterized by intense irritability and a strong inclination to quite sudden outbreaks of violence. The alien movements are often the first symptoms of impending psychic dissolution. It is now quite well established that the seat of pure chorea is in the brain, and chiefly in the grey substance of both the basal ganglia and the cortex. Its contagious nature by imitation, the close relation between motion and sensation generally, Meynert's conception of chorea as a convulsive phenomenon of irradiation of the fore-brain, the fact that choreic movements desist in sleep and are increased by mental excitement and by passion and fright, all indicate its close affinity with psychic processes. Congruent groups of muscles must constrict distinctly and in definite order if co-ordinated motions are to be produced. If the excentric impulse from the volition centres finds this plexus or series of associated constrictions broken up or inverted or unevenly interrupted, and the symptoms of chorea are present, and causes or is the concomitant of dissolutive, degenerative symptoms in the psychic zones.

Dichterische Einbildungskraft und Wahnsinn. Prof. DILTHEY. Leipzig, 1886, pp. 30.

The author opposes the commonly asserted kinship between genius and insanity. They are as unlike as the heat from healthy play or superfluous vitality and the heat of disease. A genius differs from common men in having more energy, in taking greater pleasure in his mental processes, is not pathological, but a perfect or superior type of man. His reciprocity with his environment is closer, his mental images, though vivid, numerous and spontaneous, as is shown by the interesting accounts of their mental processes given by several authors quoted, are especially distinguished, even in geniuses of the most demonic type, by being in closer and more logical relations with the environment and with each other.

Konrad Deubler. Von A. DODEL-PORT. 1886.

This remarkable work, in two large volumes, consists of day-book, biography, and correspondence of an Austrian peasant-philosopher. The son of a miner, apprenticed to a miller, later a baker, innkeeper, guide, and peasant, engaged all his life in hard manual labor and suffering manifold afflictions and indignities, he gradually,

by his own almost unaided and unschooled efforts, wrought his way to learning, gathered a library of his own slender earnings, attained singular beauty and independence of character, came into correspondence with many learned men. Engaged in pondering the highest themes while occupied with the lowliest duties, there is much in his opinions and traits which suggests Epictetus and Boehme. His portrait in steel is prefixed.

Die Bedeutung der Mimik für Diagnose des Irreseins. Von Professor Likonsky. Neurolog. Centralblatt, October 15 and November 1, 1887.

Two kinds of mimetic movements of the insane are distinguished, expressions of changed consciousness, and especially self-feeling, and abnormalities of facial innervation which have nothing to do with mimesis. In melancholia attonita the lower facial muscles are relaxed and the face seems prolonged. The corners of the mouth are drawn down. Horizontal wrinkles extend often entirely across the forehead. The mouth is shortened horizontally and slightly open. The muscles become fixed as a mask, and from contraction cease to express emotional character. In excitement they respond to emotional change very slowly. In mania the "muscular insanity" of the limbs is seen in typical cases as grimaces that do not express the emotions they would normally indicate. General excitement is also expressed in tensions that multiply wrinkles and sharpen the features. The expression of opposite emotions at the same time by different features is typical. In secondary apathetic dementia the face is smooth and expressionless, save the corrugation caused by the m. frontalis, which retains its emotional excitability longer than all others. In secondary dementia and verrüktheit there is much in common. Most interesting, however, is the mimesis of degenerative states. Here three types are distinguished: (*a*) Great preponderance of the muscles of the forehead over those of the lower part of the face. Sometimes all nuances of emotion are expressed by the frontal muscle alone in multiplying and deepening both vertical and horizontal corrugations. In other cases this muscle habitually expresses concentrated attention or meditation. (*b*) The upper lip is enlarged and is the centre of emotional expression, the excitement passing easily into irregular choreic movements. (*c*) The muscles involved in smiling may be the centre of excitement, and then those involved in sneering and crying are often involved, so that the laughter is convulsive and pathological. The eyes often sparkle, but the joy expressed seems painful. Duchenne's charts show that this involves different muscles from those involved in laughter mingled with sadness in normal cases. In all these cases the mimetic change is primary, so that emotions, even though unchanged, must work upon a changed mechanism. The mimesis is independent of will and consciousness. Relaxation and isolated partial changes in the muscular innervation of the face are also observed. After these higher psychic functions are weakened, the play of emotional expression on the face becomes more free, sharp, and intense. Mimesis of an undifferentiated character, and that involving the thick upper lip alone, are especially common among the savages, and may be called devolutive in the insane. These symptoms may be brought into relation with other expressive movements and have high diagnostic and prognostic

value. Finally, this fact is mentioned: Years ago the writer practiced constricting his facial muscles singly before a glass. He found the left side of his face most expressive and also most educable, and could do much with these muscles that he could not with those of the right side. Resuming these practices after years of intermission, he found to his surprise that he could now subject the right side to his will in what he could not do before, quite as well as the left. These isolated constrictions are possible on the lower part of the face only unilaterally, and cannot be accomplished bilaterally. Freusberg's account of anomalous movements in simple psychoses (Arch. f. Psych., Bd. XVII) and Dr. Ziehen's more special article (Berliner Klin. Wochenschr. 1887, No. 26) cover somewhat different ground, although more closely related to this work than any other recent studies, so that Likonsky's observations are to some extent novel, and it is hoped may suggest further work in the same direction.

Arrested and Aberrant Development of Fissures and Gyres in the Brains of Paranoiacs, Criminals, Idiots, and Negroes. C. K. MILLS. Journal of Nervous and Mental Disease, September and October, 1886.

This valuable article, in the form of the presidential address of the American Neurological Association, designates the marks of cortical conformation of low type as follows: Simplicity of structure, with well defined and little complicated fissures and gyres, especially the frontal; atypical asymmetry and unusual symmetry; distinctness of Benedikt's external orbital fissure; partial or complete uncovering of the insula; absence of sinuosity in the central fissure, and imperfect demarcation from the sagittal and sylvian fissures; confluence of the central fissure above, below or lateral, and perhaps confluence generally; sharp, long, unabridged parietal fissure; small marginal gyre; elongated retrocentral fissure; an occipital fissure open in the lateral surface, with the superior pli de passage below the brain level; great length of the posterior vertical arm of the supertemporal or parallel fissure, with tendency to confluence with the sylvian, occipital or parietal fissure; smallness of paracentral lobuli and precuneus, and universal destruction of the median portion of the occipital fissure. Interesting specimens are shown. There is no criminal type of brain, for crimes are of most diverse character and from opposite motives, and at least such a type if it existed would be clearly allied to the types found in idiots, inebriates, and paranoiacs. Whether fissuration be due to mechanical causes or represent lines of retarded growth, each fissure is probably not due to a distinct process, but is in many cases, as Dr. A. J. Parker had shown, due to "vegetative repetition." If thus some fissures are secondary, it is idle to seek homologues for each fissure, even in closely related brains. Dr. Mills concludes by reminding us that it is not by the study of fissures and gyres alone that the whole truth can be determined, but the depth of fissure, thickness of gray matter, quality of tissue, weights, difference in ventricles, capsules, corpus callosum, etc., should be studied and compared, and such patient work would be of great value and would yield sure results to the patient student.

Change in the Composition and Function of the Brain by Psychic Influence. By F. RICHTER. Berliner Klin. Wochenschrift, February, 1887.

The stimulus of normal psychic activities, which it is the object of psychic therapeutics to apply, may be so devised as to be a powerful auxiliary, though always subordinate to diatetic and physical means in curing certain brain diseases. This is especially the case in disturbances originating in shock, overwork, care, sorrow, losses, bad habits, and false education. All forms of psychic shock cause first local anaemia of the brain with probably less hyperaemia in adjacent regions. Cohnheim and Arndt hold (and the former claims to have experimentally demonstrated it) that repeated stimuli cause contraction of capillaries, and that if this has lasted long its cessation leaves the porosity of the capillary walls impaired so that the blood elements too freely saturate the brain and thus impair its functions. This unequal distribution of blood affects the vasomotor sphere in turn, and arterial pressure and transudation and imbibition ensue. Arndt believes that the ganglion bodies thus tend to lose their processes, become apolar and even indistinguishable from adjacent nervous tissue, although, as Richter suggests, this begins to look like the results of inflammation. Such changes are ascribed to abnormal or excessive psychic stimulus, and the symptoms which attend them resemble those which follow cerebral neuroses of anaemic and dyscrasic origin, and may be attended by hemianaesthesia, neuralgia, exhaustion, lameness, cramp, aphasia, cardiac neuroses, nervous catarrh of nose, stomach, intestines, nervous metritis, irritability, depression, etc. But mental hygiene, wisely directed, has a regenerative influence. A deranged cortex with false psychic functions may have its abnormal tissue or compounds degenerated or decomposed by wise psychic regime. Morbid inhibitory stimuli may be neutralized by normal stimuli. For abulia with consequent lameness, excitement of the will is prescribed. Paralyses from fright are redressed by new psychic shock. The greatest tact is of course needed in such cases to hit the right nuance between sedative and stimulating influences and decide on just the right psychic state to neutralize the morbid one. The greatest personal ascendancy over the will and even imagination of the patient, and with of course isolation from too tender friends, is indispensable. Narrow-mindedness, conceit, stubbornness, and in fact pure psychoses generally, are harder to deal with than neuroses with psychopathic symptoms, and require ascendancy over the mind of the patient and an ability to impose a good psychic sphere, which makes the highest degree of confidence on the part of the patient absolutely indispensable. The weak point of this paper is the absence of indirect proof (direct being of course out of the question) of the underlying assumption of positive regeneration or "Rückbildung" of cell processes or other brain tissue.

On Changes in the Nervous System after Amputation of Limbs, with Bibliography and Recent Cases. E. S. REYNOLDS. Brain, January, 1887.

The conclusions of this valuable and comprehensive paper are that the numerous small fibres of the sciatic trunk after amputation are results of atrophy and not degeneration. This is ascribed to disuse only, connection with the trophic centres preventing degenerative

change. Most of them are demonstrably sensory and could be traced through the posterior spinal ganglia to the cord, but some are as certainly motor. Afferent impulses from parts removed are of course impossible, but motor impulses overflowing from the cord to stumps, though only to be blocked at the site of amputation, are at least conceivable. The small fibres caused by general paralysis must be distinguished from Ranvier's small fibres constituting the neuroma and occurring at the end of the central stump of a cut nerve. The postero-lateral group is not sensory, but motor, innervating the muscles which maintain the erect position. Affection of the sensory tracts reduced the size of the posterior column and horn of the same side, but Clarke's columns were intact, their function being, as Gaskel has almost conclusively shown, the innervation of the viscera. All shrinking due to amputation is compensated by great widening of the lymph channels and slight increase of connective tissue in the small bundles.

Ueber Koprostasic-Reflex Neurosis. By Prof. E. H. KISCH. Berlin. Klin. Wochenschr., April, 1887.

Neuroses of the heart are the most common of the reflex neuroses, which the author thinks to be duc to habitual constipation. Next in order of frequency follow hemicrania. Then come sciatica, lumbar-abdominal neuralgia, ovaralgia, and the trigeminal neuralgia of Gussenbauer. The author feels justified in designating these as a distinct group of neuralgic affections due to defective action of intestinal ganglia, or in the terms in which Nothnagel summarized the results of his investigations, to "a diminution of the automatic activity of the nervous apparatus of the intestines."

Ueber die posthemiplegischen Bewegungsstörungen. Eine klinische Studie. B. GREIDENBERG. Arch. f. Psychiatrie, 1886, p. 131.

This extended study, with very copious use of the literature of the subject collected in 267 titles at the end, in this new and fruitful field, is too crowded with details to be adequately reviewed. The main result reached by the author, not only from the literature but from careful study of cases, is expressed in the following table classifying post-hemiplegic movements:

- Contractures
  - apoplectic, cramps, clonic
    - tonic
    - intermittent
    - muscular rigidity
  - early — paralytic, passive
  - late
    - constant, lasting, fixed
    - changeable, (latent)
- exaltation of tendon reflexes
- co-ordinate movements
- tremors
  - reflex, clonus
  - essential
    - trembling proper (tremor) in the form of paralysis agitans
    - or of disseminate sclerosis
- hemichorea
  - constant
  - with intended movements, disturbance of co-ordination (hemi-ataxia)
- athetosis

} Composite forms in various combinations.

Un Cas de Dégénérescence psychique héréditaire. Par Dr. JAKOWLEW. L'Encephale, 1887, No. 2.

The term "hereditary psychic degeneration" is proposed as the designation of a new nosologic group. Next to heredity, neurasthenia is the cause of most psycho-neuroses. The many phobias, which since Kowalewsky's work in 1885 are generally thought to be at bottom identical and named pathophobia, are among the most common indications of hereditary taint. The case, which is described at length, is of a prompter in a theatre aged thirty-six, and is of great interest and carefully studied, including compass and electrical measurements. The conclusions drawn by the author are that pathophobia, a sense of being possessed, and impulsive actions are products of the same conditions, and are attended by terror; that the patient is fully aware of the absurdity of his ideas (which circumstance distinguishes these cases from similar phenomena in cases of primary insanity), and that all these phenomena are connected with nervous weakness.

Des Intervalles Lucides, considérés dans leur rapports avec la capacité civile des aliénés. E. RÉGIS. L'Encephale, No. 2, 1887.

The unsatisfactory nature of present French legislation on the relations between crime and insanity has been recently pointed out in an interesting series of articles in *Le Progrès Médical* for the current year, by the magistrate, A. Martin, in *L'Encephale* and elsewhere. One of the most vital points for the safety of society, as is well known, is the conception of lucid intervals, of which Dr. Régis distinguishes three kinds — remission or attenuation of the more marked symptoms, complete momentary suspension of symptoms, and intermission or a complete return to the normal state between two attacks. The distinction between remission and intermission is especially insisted on. Both as to the nature and duration of the intervals French law is inferior to that of ancient Rome. A permanent departmental commission should consist of a doctor and an officially subordinate administrator to minimize the present difficulties.

Diseases of the Nervous System. Vol. V of A System of Practical Medicine by American Authors. Edited by William Pepper, M. D., LL. D., assisted by Louis Starr, M. D. Philadelphia: Lea Bros, 1886. pp. 1317.

This large volume is made up of contributions from twenty-three eminent American practitioners, including Drs. Robert Edes, C. K. Mills, Weir Mitchell, J. J. Putnam, E. C. Seguin, E. C. Spitzka, Allen Starr, and H. C. Wood, and contains nearly sixty diagrams. The articles on general semiology, localization, mental diseases, hysteria, hystero-epilepsy, catalepsy, ecstasy, and disorders of speech are of especial value for students of psycho-physics. These topics, as well as many others treated in this volume, have now come to be represented by so voluminous a French or German literature, that even special students in the neurological field are bewildered in seeking a serviceable knowledge on these themes in continental publications or clinics. Such guidance this book is intended to afford. To specialists, as well as to the general practitioner, this volume is far the best in this field in English. That there should be repetitions, as well as great inequalities of merit in the different

parts, is not surprising on the plan adopted. It is greatly to be hoped that such a book may help to improve the quality and increase the amount of instruction given upon these topics in our medical schools. That but seven pages, and those not abreast of our present knowledge, are given to disorders of speech, and that nothing is said of hypnotism save very incidentally in the chapters of Dr. C. K. Mills on hystero-epilepsy and ecstasy, are defects, the one in the execution and the other in the plan of this volume, which we shall hope to see remedied in a second edition. Few chapters will be of greater value and interest to psychologists than that on Mental Diseases by Dr. C. F. Folsom, one of the most distinguished specialists in New England, and which is reprinted by itself.

Mouvement de l'Aliénation Mentale à Paris, from 1872 to 1885. A. PLANÈS. Annales Médico-psychologiques, January and March, 1887.

These statistics show a gradual increase of insanity, in proportion to the population, as measured by entrance to institutions. More men than women are afflicted, and most attacks are in June, and least in February and September. The most rapid decrease is from the middle of August to the middle of September, and the most rapid increase is from the middle of February to March and from the middle of April to May. From September to October considerable increase occurs, followed by a no less considerable fall to November. We cannot follow here the nine different forms of mental alienation for each sex which are clearly presented and suggestive.

Alternation of Neuroses. G. H. SAVAGE. Journal of Mental Science, January, 1887.

Not only do different forms of nervous disorders appear in different members of the same family, but epilepsy, insanity, depravity, idiocy, and somnambulism may be represented in the children of the same parents. Headache often alternates with insanity. Hysteria alternates with various neuroses; epilepsy, even asthma, disappears on an outbreak of insanity. Rheumatic fever and insanity are often associated alternately, and many functional troubles are relieved by bodily disease, on the principle designated in a late German essay as "kinetic equivalence."

A Manual of Diseases of the Nervous System. W. R. GOWERS. London, 1886.

The introductory chapter to these two volumes distinguishes four pathological classes of disease. 1, Coarse organic diseases, such as hemorrhage, softening tissue, etc.; 2, structural disease; 3, nutritional disease, chorea, and general paralysis, which latter the author judges so without structural pathology as to belong in neither the first nor second class. The chapters on general symptoms and on electrical excitability of nerves and muscles follow. Part first is devoted to diseases of nerves. In the section on neuritis and morbid growths, sciatica, to which an entire chapter is given, is called not a neuralgia but a neuritis. Of the five classes of multiple neuritis, the tabetic form only is provisional, and its relation to chronic alcoholism is undoubted.

Zur diagnostischen Bedeutung des Pupillenphänomene, speciell des reflectorischen Pupillenstarre bei Geisteskranken. Dr. THOMSEN. Charité Annalen, 1886, p. 339 et seq.

On a basis of observation of 1700 patients in the insane department of the Charité, the author concludes that most cases of reflex pupillary rigidity are paralytic, and that it is of much importance because it is sometimes found at a stage of the disease where other symptoms are wanting. Besides paralysis, it also occurs with aged dements, chronic alcoholists, in cases of lues, or lesions of the head without other symptoms of organic lesion of the brain, and sometimes, as with sane patients, with tabes, multiple sclerosis, paresis of the oculomotorius, cerebro-spinal meningitis, etc.

Pupillenreaction und ophthalmoscopische Befunde bei geisteskranken Frauen. Dr. SIEMERLING. Charité Annalen, 1886, p. 363 et seq.

These results are based on observation of 923 cases, and paralytic dementia furnishes by far the greatest number. One interesting case of hysteric origin was observed, but functional psychoses furnish but very few cases.

Psycho-Therapeutics. I. LESLIE TOLEY, M. D. (London). Am. Journal of Insanity, April, 1887.

This writer believes that "in the near future the general practitioner will pay closer and more systematic attention to that all-important branch of medicine—mental therapeutics." He will enter into the patient's tastes, sympathies, foibles, and the different powers and phases of his mind. The personal influence and manner of the physician, always so important, is chiefly so in nervous and mental cases. Pure and lofty sentiments are directly conducive to bodily health and vigor. Cheerfulness, art, literature, friendship, industry, proper employment, religion, music, change of scenery, good weather and climate—the influence of these is vast, and is likely to be recognized more and more, though by no means to the exclusion of the *materia medica*.

Observations with Sphygmograph on Asylum Patients. By T. D. GREENLEES. Journal of Mental Science, January, 1887.

So far from insanity having no pathology, as is often said, mania, melancholia, epileptic insanity, general paralysis, dementia, and imbecility show distinct and characteristic tracings, according to this observer, illustrations of which are printed.

L'auto-intoxication dans les Maladies. Par M. BOUCHARD. Paris, 1887.

The author shows by convincing experimental demonstration that the healthy body makes and eliminates poisons. The toxic power of normal urines is most clearly shown.

General Paralysis of the Insane. W. J. MICKLE. London, 1886.

This second edition of the above book is now without question the best repository of what is known of this disease—its history, literature, symptoms—that exists in English.

IV.—MISCELLANEOUS.

Pictographs of the North American Indians. A preliminary paper by GARRICK MALLERY. Fourth Report of the Bureau of Ethnology, pp. 1–256.

Taken in connection with the author's elaborate paper on sign language among the North American Indians, in the first volume of these Reports, and his other papers in progress but not yet published, Mr. Mallery's work is indispensable for all who are interested in expression of psychic processes by means more primitive than speech and writing. These studies are already so far advanced that types of execution may be expected to disclose data for priscan habitat and migration, as within each tribal or other system every Indian draws each figure in identical style. In this report nothing is attempted but to furnish a repertory for points on pictographic representation of ideas. The distribution of petroglyphs is very wide, from Eastern Canada to California, and extends far into South America. Pictographs on bone, wood, skins, gourds, and the human person, tattooing, the significance of colors, mnemonic quipu, etc., are discussed as illustrating the evolution of pictography. Then follows a very full explanation of the famous Lone Dog winter-count from 1801 to 1873, and of the still older count obtained by Dr. Corbusier which extends back to 1775, in which each year is marked by a pictograph of some distinctive event. Pictures of each of eighty-four totemic personal names of the Ogalala roster, and of two hundred and eighty-nine in the Red Cloud census, are given with English names appended. The symbolism of feathers, differently tufted, notched, painted; mystic personages, charms and fetishes, Shamanism, mortuary emblems, grave-post markings, pictographs illustrating daily life, tribal history, individual biography, modes of interpretation, frauds, and practical suggestions to collaborators, are topics which receive less attention. It is remarkable that no writer on this subject has extended his ken to take in and to attempt to co-ordinate the very different and independent work of the few chief writers, Darwin, Delsarte, Mantegazza, Clark, Mallery, and Warner.

Das Wachstum der Kinder. Prof. GAD. Humboldt, January, 1888.

After a short account of the special investigations on the topic of the growth of children since Quetelet, Dr. Gad proceeds to a convenient résumé of the very extensive investigation begun in 1882 by Malling-Hansen, director of a deaf-mute institute in Copenhagen, and now published in instalments. A system of weighing and measuring, daily and sometimes several times daily, was developed and applied, not without much expense, to 130 children in a way which inspires much confidence in the results, which are briefly as follows: Changes in the weight of children from nine to fifteen years are subject to three annual variations, viz. a maximal period, from August to the middle of December; a middle period, from the middle of December to April; a minimal period of three months, from the end of April to July. During the first period the increase of weight is three times as great as in the second, and all that is gained in the second is lost in the third period. There are three periods also of growth in height, viz. the minimal, from August to

the end of November; the middle, lasting to the end of March; and the maximal, from the last of March to the middle of August. The daily growth in height is twice as great in the middle as in the minimal period, and two and a half times as great in the maximal as in the minimal period. Thus the growth period extends from the end of March to December, and falls into two parts—first, the maximal period of height and then that of weight. Thus the minimal period of weight falls in the maximal period of height and *vice versa*. Increase of weight grows suddenly from a minimum to a maximum and then slowly declines, while increase of height comes on slowly and declines suddenly. In the maximal period of increasing height, growth in thickness is at its minimum, and conversely. He then infers that as much as possible of the periods of growth in both height and weight should fall for school children in the summer vaction. Besides these annual phases, these studies reveal growth periods of 25 and also of 75 days. Whether these are due to local meteorological conditions, as sun-rotation periods of 27 days, which Malling-Hansen calls growth energy, is not clear. The history of daily variations, especially in height, due in part to compression of cartilages and loss of elasticity of the arch of the foot caused by standing, and the converse effects of rest, is given with great detail for every hour of school life, and is full of interest. The author believes he has only obtained a very inadequate glimpse of a wide and rich field of research.

Le Surmenage Scolaire. Par CH. FÉRÉ. Le Progrès Médical, February 5, 1887.

The writer starts with the dictum of Spencer that the first condition of national prosperity is that the nation be formed of good animals. Sedentary life and intellectual work have always tended to become unnatural. Tissot, in his acute work on the health of men of letters, written in the last century, and Réveillé-Parise, in his book on the physiology of men devoted to mental work, Rousseau, and more recently Lagneau and Dujardin-Beaumetz, have directed public attention, with increasing explicitness, to the dangers of sedentary mental work, which are: I. Those due to unfit places. The site is often badly chosen. If the rooms are small there is more danger of contagious affections; insufficient air gradually impoverishes the blood, and anaemia, chlorosis, depression, bring receptivity to all morbific influences. II. Haste in eating, bad cooking, and food unscientifically chosen, cause defective nutrition of some parts or organs of the body. III. Clothing is often unpedagogic in form or thickness. Because of the proportion of surface to mass of body, children, it is known, lose relatively more heat than adults, and it is often forgotten that clothing is to an extent a caloric equivalent of heat, and that brain-workers need to dress warmer than muscle-workers. IV. Insufficient exercise brings constipation, then slow blood and nutrition so ill adapted to growth that even the teeth are starved into bad development and piles and incipient sexual weaknesses appear. V. Excessive labor is laid on the eyes and sometimes the fingers. VI. Bad attitudes. The race has hardly had time to adapt itself to sedentary intellectual life. Lying on the back is particularly favorable to brain circulation. According to elaborate statistics by Guillaume, the percentage of girls and boys who are more or less deformed by bad attitudes is

forty-one and eighteen respectively. Besides three distinct species of lateral curvature, all largely due to unhygienic attitudes in writing, other thoracic deformities are induced, the effect of all of which is to reduce the vigor of respiration, circulation, and impair nutrition and growth. VII. Vicious habits. Prolonged sitting favors pelvic congestion and local irritation, strongly inclining to masturbation, to which the mental anomalies of deterioration strongly predispose our degenerate youth. Since Tissot (L'onanisme: Œuvres, T. 1), many psychic and somatic troubles have been attributed in form to secret vice. The result is general exhaustion, causing troubles of nutrition, circulation, memory, and depression, irritability, fluctuations of mood, etc. VIII. Excessive mental labor. Moreau's "irritable diathesis" seems increasingly often caused among those striving to acquire culture in the lower classes, and the somewhat greater liability to this form of degeneration among the upper classes is due to heredity. In general, subjective sensations are more vivid in fatigue. Nervous exhaustion tends to depression, which precedes most vesanias. In fine, all these causes together are tending to that form of degeneracy which is incapable of productive effort.

The Children. How to Study them. By FRANCIS WARNER, M. D. London, 1887. pp. 80.

These lectures, given to the Froebel Society, are especially devoted to ways of observing nutrition, eyesight, facial expression, gesture, and posture. The functions of the arm, hand, and spine are especially to be scrutinized, and points to be observed during sleep are enumerated. Some of the cuts and conclusions of the author's work on physical expression, described in our last number, are reproduced.

Tachyhippodamia. By WILLIS J. POWELL. Reprinted in the Southern Live Stock Journal during July, August, and September, 1887.

This rare and often vainly-sought handbook, privately printed in 1838 and sold at a high price as the revelation of a valuable secret first discovered by the author in 1814 and perfected during twenty subsequent years, is here for the first time accessible to the general reader. The author was at first a teacher of Greek, Latin, and modern languages, who later acquired a fortune by this art of taming wild horses freshly caught from the plains of Texas and Mexico, in from two or three to six or eight, or in rare cases twelve or even sixteen hours, all without the least violence. The tamer approached the horse which had been driven, led, or dragged with the lasso into a small enclosure. The animal first turned his tail to the trainer, but in fifteen minutes or half an hour turned about. By motions so slow as to be almost imperceptible the hand was extended and the man approached, stopping instantly at the faintest sign of flinching or fear. At length the nose could be touched and tapped or patted by very slight but rapid movement. Inch by inch this "gentling" process proceeded to the neck, body, fore and hind legs, to the feet, tail, ears, etc., till the horse had been handled all over. All animals have much pleasure in dermal sensations, for the sake of which they will endure more and more sudden and violent aural and optical sensations, and these are the best means of removing fear, which

is the only feeling a confined animal has for man—these are his fundamental principles. An animal that rushes toward a man and would kill him if he fled or showed signs of fright, will always stop a few feet from a man who remains motionless, if there is no distracting object, so that the attention of the animal is fixed on no moving or sounding thing whatever save the trainer. As there are irregular verbs and nouns that do not fall under the paradigms, so he says there are exceptional horses, but none he thinks which variations of this method will not subdue. The secret, he argues, is the moral one of gradualness, gentleness and perseverance, and inspiring confidence, and all drugs, smells, violence, or magnetism are methods of quacks ignorant of the true psychic nature of the horse. Very interesting are the details of "gentling" a wild boar of great ferocity, freshly captured and uninjured, which could at first be only gradually touched with a stick through the bars of a pen, and of a freshly caught adult deer, both of which were so tamed in a single day as to eat out of doors and in public from his hand. Many certificates from military and civil officers of highest rank certified to the marvels of his art and the permanence of his results. The book is written in an accurate and naive way, with incidental allusions to learning and educational art, which gives it, though in slight degree, something of the charm of Isaac Walton, or White of Selbourne. This seems another illustration of the law that very great changes of impressions, whether of pressure, heat, or cold, can be accomplished without consciousness if they are sufficiently gradual—a law of wide range and great utility in education.

Le Leggi statistische del Suicidio secondo gli ultimi Documenti. MORSELLI. Milano.

This work is a continuation of the author's treatise on the same subject published in 1879, and is one of the very best illustrations of the exact methods of the anthropological school of psychology. The regularity of increase of the number of suicides, which constitutes one of the best arguments for the doctrine of determinism, is much greater than the increase of population. The larger the town the greater the annual increase. The yearly variations depend on cosmic, social, meteoric, and economic changes. The two zones of greatest frequency of suicide are the centre of the German population and Northern France. From these in all directions the frequency of suicide decreases like waves from a stone thrown in the water. This geographical scheme repeats itself on a smaller scale for other smaller centres, the great cities exhibiting, of course, the largest percentage, these latter and race being the chief factors. Like outbreaks of insanity and crime, suicides increase during months of increasing temperature. Spring, summer, winter, fall, is the series which represents decreasing numbers of suicides, June being the month of most and December of least frequence. Suicides increase with culture and civilization. In Italy about twice as many suicides occur in cities as in the country. The state of religious consciousness has a great influence on the tendency to suicide, which is strongest among Protestants; then follow Catholics, Jews, Mahomedans, fetish-worshippers, in decreasing series. Men are about four times as likely to commit suicide as women, but the percentage of women to men is greater in spring and summer, and of men to women in fall and winter. Each race and nation, how-

ever, has its own distinctive peculiarities in this regard. Liability to suicide increases regularly with age, and reaches its maximum by women earlier than by men, being great for both about the involution period. Unmarried life, especially the state of widows, widowers, and divorce, favors suicide, family life tending strongly against it, and widowers are more exposed to it than widows. Children are one of the greatest protections, especially for women. The well-to-do classes are more exposed than the poor. Among vocations, soldiers, and most the older subordinate officers, exhibit the highest percentage. The means of suicide vary regularly with the season of the year, race, climate, and culture. In Russia, Norway, and Prussia, hanging is decreasing and drowning increases. In Denmark, Belgium, Holland, and France, the reverse tendency is exhibited. Death by firearms is steadily increasing; by charcoal fumes decreases in France and increases in the west of Europe, especially in great cities. In northern lands hanging is the mode of death in three-fourths of all cases, while in the south drowning is more the fashion. Italians often precipitate themselves from precipices, and Anglo-Saxons often stab themselves. More painful and uncertain modes of death are rapidly declining. In Italy men choose firearms, women drowning. In Austria men prefer hanging. Unmarried women and servants prefer poisoning, and in each land each age has its preferred mode of death. Suicide is largely an index of social misery, and corrective influences are to be sought partly in the reform of popular concepts by philosophers and moralists, but also in a social reform which shall establish a better equilibrium between individual needs and the possibilities of social development.

L'Anomalie du Criminel. R. Garofalo. Rev. Philos., March, 1887.

There is a class of criminals who have psychic and often anatomical anomalies, not so much pathological as degenerative or regressive and even atypic in character. Some have traces of arrested moral development, although their faculty of ideation is normal. Others have instincts comparable to those of children or savages, are deprived of all altruistic sentiments, and act only under the empire of their own desires. These anomalies are absolutely congenital, and not produced by social or other environment, so that society has no duty to them whatever but to repress them. These are totally incapable of adaptation, and represent a source of continued danger to every member of society.

Le Délit Naturel. R. Garofalo. Rev. Scientifique, January, 1887.

What among the crimes of our day have always in all times and places been considered punishable? These acts are natural crime, judged from the sociological standpoint, and are opposed to the average moral sense of the entire community, from which laws and ethical systems and commercial ideas of right spring. There can be no exhaustive and definite catalogue of such acts made out, but it can only be concluded that all crimes are violations of one of the two great altruistic sentiments. The first is pity, sympathy, or humanity, and includes now defamation, assault, injury to the physical or moral health of children, etc.; and the second is the sentiment of honesty, including also a long list of special crimes, such as

theft, incendiarism, plagiarism, false testimony, etc. This does not include acts against the state, which vary with the particular conditions of nations. These sentiments are the substratum of all morality, and their absence, which is as much an abnormality as the privation of a limb or physical function, constitutes two distinct types of mental alienation.

NOTES.

I.—EDUCATIONAL.

An attempt has been made recently, in the State Normal School at Worcester, Mass., to enlarge the scope of the ordinary study of psychology, as well as to render that study more objective and more useful to students, by making the systematic observation of children a part of the regular work of the school. The object is twofold: first, to put the students (as prospective teachers) into closer and better relations to children; and secondly, to gather a store of well ascertained facts wherewith in time to increase and rectify our present unsatisfactory knowledge of child-nature. The mode of procedure, hitherto, is somewhat as follows: First, the aim and methods of the study are carefully explained to the students at the beginning of their second half-year in the school; they then improve such opportunities as they have or can find—at home, in the street, in the families of neighbors and friends—of noticing with care and minuteness the spontaneous and unconstrained activities, bodily and mental, of children of all ages—at play, at study, at work, in conversation and intercourse with each other and with adults, in all situations, relations, and moods. Then, at the earliest convenient moment, careful and concise record is made, on blanks provided for the purpose, of the facts observed, these being kept as free as possible from any reflections or inferences of the observers. The blanks are ordinary half-sheets of note-paper with printed heading, giving date, observer's name, initials of the child observed, its sex, nationality, age, and the length of time between making the observation and recording it. Different tints of paper are used as an aid to classification; for example, white for ordinary personal observations, cherry-tint for hearsay or second-hand facts, canary-tint for reminiscences of the recorder's own childhood, chocolate-tint for observations continued without break for a certain specified length of time, and so on.

The number of observations recorded varies, of course, from week to week with the opportunities that present themselves, but a rough average would be not far from two a week for each pupil. During the two years that the experiment has been in progress in its present shape, somewhat more than four thousand eight hundred records have been made, and these have been carefully classified by subjects and preserved for reference. Many of them, from lack of skill or judgment in the observer, have little value apart from the wholesome endeavor that made them, but a considerable proportion are of permanent interest and significance to any student of child-nature. Taken as a whole, they already form a body of facts not to be found elsewhere, and the practice by which their volume is continually increased also improves their quality. They relate chiefly to the knowledge and ignorance and errors found in children of different ages; to their

instincts, as manifested in play and in voluntary occupations; to the abilities of children in various directions, as shown in drawing, mechanical construction, hunting and training animals, letter-writing, rhyming, story-telling, etc.; to their feelings (hundreds of records covering a very wide field), memory, imagination, attention, moral sense, idiosyncrasies, etc., and are classified according to such rubrics.

In addition, considerable information has been collected bearing upon the treatment of children (injurious or otherwise) by adults—parents, teachers, nurses, grown-up brothers and sisters, etc., and also upon the kinds of literature, stories, pictures, songs, and the like, that children enjoy most and remember longest. As to the good effect, as training, of these observations upon those who make and record them there can be no question whatever. The students soon become noticeably more interested in children and their ways, and more skilful in dealing with them, while certain individuals acquire much tact and ingenuity in following out the more complicated and obscure processes of child-life. Moreover, they get some good practice in right methods of observation and investigation generally, learning in some measure the caution, discrimination, and veracity required in studying nature. The exercise, as a whole, stimulates and quickens. Students do not find it a dull task. They have to be restrained, or they would have given a disproportionate amount of their time to it. Finally, graduates who have had a year or two of this training before going out to teach, manifestly take more pleasure and are more successful in their work in consequence. They frequently fill long letters with accounts of the interesting traits they discover in their pupils, and it is easy to see that their attitude towards exceptional and troublesome children is often marked by unusual intelligence and sympathy.

How radical a modification of methods of teaching psychology is here involved is evident. The "natural" method, which has slowly reconstructed modes of teaching all other subjects during the last century, has at last reached the science of man. This is the field work of psychology. The following commencement essays of graduates last summer were based entirely on these studies: Falsehood in Children, Likes and Dislikes of Children, The Laughter of Children, What the School Child Thinks Of, How a Child Reasons, Plays of Children, Superstitions of Children, Study of a Child. A brief digest and tabulation of results of the above records, which are not without scientific value, will appear in the Journal later. Great credit is due to Principal Russell for the skill with which he has organized this significant new departure.

Several very recent investigations show that some children lack the power to distinguish shades of sound, both vowel and consonant, and hence are capable of quite a range of distortion of sounds. This does not seem due, at least in some cases, to defective hearing, and hence the term "sound blindness" more often used, or "timbre deafness," which Professor J. Le Conte suggests as more apt. From the many illustrations of the defect cited it would appear that defective carrying power of memory has much to do with it. This seems to be one cause of the great difference between children in learning to read, but the phenomena need fuller study than they have yet received. Not a few of the transformations and mutilations of words reported would make excellent stock in trade for low comedy as they stand.

Dr. W. Camerer, whose long experimental studies on the sense of taste, with his wife, are of such high significance, now reports (Zeitsch. f. Biolog., Heft 2, 1887) a careful study of two years' duration on the metabolisms of five children between the ages of seven and seventeen. During this period their food was observed and within wide limits regulated, their weight and growth recorded, excretion and even insensible perspiration registered at intervals. The results are too extended for report here, but the role of individual differences, especially of sex, the range of individual peculiarity in distribution of excretive function between bowels, kidneys, and skin, is surprisingly great.

An important hygienic educational address was given before the Berlin Medical Society, February 16, 1887 (Berlin. klin. Wochenschr., March 7, 1887), by Dr. Gehrmann, on "Insufficiency of the muscles of the trunk." The cause may be due to the muscles themselves, or be reflex, perhaps from intestines, womb, etc., and may be general or local. The position in sleep is of great importance for giving physiological posture. The results of defect are scoliosis, bilaterally asymmetrical growth, wandering liver, sinking kidneys, falling womb, too feeble action of one or both lungs and resulting fluxions, and irregularity and disease of the heart.

A most instructive case of hereditary juvenile degeneration is described by Mabille and Ramadier (Annales Méd.-psycholog., May, 1887). A boy whose neurotic parents felt schooling to be the chief end of life was isolated that he might learn more and quicker. Although industrious and ready of apprehension, he grew gloomy as adolescence approached. At eleven he was placed in a school where his reserved ways excited derision, which led to delusions of persecution. All acts in his environment had reference to him. Gradually mystic, erotic and demonic hallucinations developed, which were, however, mitigated by an operation for phimosis. Zoophobia was so intense that the sight of hens, cats, etc., caused pallor, tremor, etc. All these symptoms soon ended in rapid dementia.

Dr. A. Stewart's recent book on "Our Temperaments, their Study and their Teaching," is an excellent illustration of the revival of the theory of temperaments on a more scientific basis as the doctrines of phrenology decline. His book is designed as a practical guide, is very rich in literary illustrations, and tabulates the physical and mental characteristics of the four pure temperaments. The latter are considered as valid only to civilized, and chiefly for British races. The book is richly illustrated. The scientific plane of the book is about like that of Mantegazza's recent work on physiognomy and gesture. This, considering the obscure nature of the subject, is high praise.

A teacher of deaf-mutes has carefully counted the words used by deaf-mutes per day, and finds that, making allowance for abbreviations, scarcely more than a thousand are used, which is probably very far below those used by normal children.

No less than eight interesting cases have been lately reported in *Science* of sudden amnesia from shock or accident which remained

after consciousness was regained. Often all that preceded the accident by a few moments, hours, days, weeks, or even more, was permanently lost from memory. In one case at least, as perfectly normal health was slowly restored, memory of events came down to a point of time nearer and nearer the instant of the accident. In some cases there seems to have been some proportion between the length of the period of unconsciousness and the memory-blank before it. One writer thinks the memory is more likely to come down to the instant of injury if the latter deeply involves the senses, especially sight.

M. Ribot contributes to the October and November issues of his *Revue Philosophique* a very convenient summary of the scientific doctrine of attention. M. Ribot brings into prominence the distinction between spontaneous and voluntary attention. The former is guided by natural interest, by the most impressive sensations, and is well marked in children and in animals. The latter is a product of civilization and is an artificial process. Attention in any form is an unnatural state. It is a monoideism, while the nature of thought consists in a constant change. Attention is based upon emotion, and its genesis must be connected with role of pleasure and pain in the struggle for life. The method of inducing voluntary attention is by appealing to emotional motives, by substituting a mediate unattractive good for an immediate attractive one. M. Ribot also enrolls himself amongst those who regard motion as the essence of attention. Without motion thought is impossible, and all thought is initial action. J. J.

Mme. Clemence Royer contributes a very interesting article upon the notions of number in animals to the *Revue Scientifique* of November 19. Her main thesis is that animals have a good sense for forms and sizes of groups of objects, but that real counting is very limited, and the idea of "three," for example, as an abstract numerical notion is beyond their mental horizon. The trained dog does not appreciate the meaning of the numbers that he pretends to add, but regards them merely as an artificial means of gaining his master's approval; just as Sir John Lubbock's dog regards the labels that he brings when he wants something to eat or to go out. Animals are good geometers but poor arithmeticians. Geometrical notions are the more elementary of the two, and it is a product of civilization that has led us to substitute number for measure; to count instead of estimating "bunchwise," as do the uncivilized. Number is a perception to animals; it is an idea to us. J. J.

II.—EXPERIMENTAL.

E. Fischer and F. Penzoldt report a study of the sensitiveness of the sense of smell (Liebig's Annalen, Bd. 231, 1, s. 131) as follows: In an empty room of 230 cubic metres content, a weighed quantity of substance dissolved in alcohol was sprinkled by a simple atomizing apparatus. The air of the room was mixed with a great fan for ten minutes, and the subject whose sense was to be tested was called in. The most striking result was that mercaptan was perceived in volumetric proportion to air of one to fifty thousand million. Assuming 50 cubic cm. of air to be inhaled, so small a quantity as

$\frac{1}{46,000,000}$ milligram of mercaptan is perceived. According to Kirchhoff and Bunsen, it requires $\frac{1}{1,400,000}$ milligram of soda to be perceived in the spectroscope.

Dr. Fauvelle thinks that there is an inverse ratio between smell and sight. In some forms of life the olfactory organ precedes all other parts of the body and becomes very mobile. Extreme prominence of the naso-labial organ not only limits the field of vision, but in some way is unfavorable to the highest development of the visual function. As the eyes acquire parallel axes and reach their highest perfection, the nose retires from its prominence in position and function. This may be true also of individuals and races.

Wendenski and Professor Henry P. Bowditch, of Boston, by different methods believed they had proved that, exceptionally to the general law that every tissue is fatigued by work, the nerve fibre in a nerve-muscle preparation was not exhausted by very long continued activity, and concluded that its function was approximately analogous to that of a metallic conductor. Professor Alex. Herzen (Arch. des Sciences phys. et natur., September, 1887) thinks he has proven conversely that when the muscle ceases to react to the stimulus of a prolonged tetanizing current its nerve is fatigued, while the peripheral end apparatus can continue to functionate.

Dr. J. M. L. Marique's thesis, entitled *Recherches experimentales sur le Mécanisme de Fonctionnement des Centres psycho-moteur du Cerveau*, though presented in 1885, deserves mention here for its admirable summary of researches on the excito-motor area and sensory centres of the cortex since 1870, and also for his novel method of experimentation, which, however, itself needs further study. He attempted to isolate the motor centres for limbs in the dog from the rest of the cortex by a vertical cut seven or eight millimetres deep around the sigmoid gyrus, severing thus, as he thinks, the arcuate association fibres without injuring the projective, or at least the pyramidal fibres. His conclusion is that section of the association fibres produces about the same result as severing the pyramidal fibres themselves, or that motor centres have no function in the absence of sensation.

Some of our readers will recall, as does the writer, an American who gave a few exhibitions of the remarkable power of not only playing different melodies of very different rhythm simultaneously with the two hands, but of writing with great rapidity, *e. g.*, a French madrigal with one hand while the other was writing a German sentence from Kant, a Psalm in Hebrew, etc. M. Paulhan, a French psychologist, has lately studied on himself the power of the mind to attend to two things at once. When he wrote the words of a poem while reciting another, the words or even letters of the two would occasionally get mixed. The confusion caused by repeating one poem aloud while mentally rehearsing another caused still more mixing. He timed the most rapid multiplication of a row of figures by two when done alone and the time required to repeat a poem by heart, and then found he could do both together in some-

what less time than the sum of the times of each separately. The simpler and more unlike the two processes the more nearly could both be done in the time of one, but very complex and similar acts cause much interference and loss, which is still greater if three things are attempted at once, as writing a poem with one hand and numbers with the other while repeating a song. The theory that in "double acts" the attention flits is not favored by these observations.

Interesting experimental investigation of the question whether after the brain had lost its function by sudden total anaemia its function could be restored by a supply of fresh blood, is reported by G. Hayem and G. Barrier (Arch. de Physiol. 5, 1887). Twenty-two dogs were decapitated, and from one second before to twelve minutes after the operation undefibrinated blood from a living horse was transferred into both carotids by a T-tube. If transfusion occurred after the head had become still, about two minutes after decapitation, respiratory movements, the corneal reflex, the secretion of saliva and tears were restored, and but twelve minutes after the knife fell the power of reviving any of these movements was gone. If transfusion took place four seconds after decapitation, the ordinary spastic movements ceased and apparently voluntary movements began again. Five or ten seconds after, while the voluntary movements of the head could be revived, the spastic motions could not be repressed. The latter, which generally cease in about ten seconds, can be restored if transfusion is made at once after their cessation.

A. König, in an article on Newton's law of color mixture and some recent experiments of E. Brodhun (Sitzungsber. der Berliner Akad. 1887, XVIII), urges that the principle that colors that look alike give mixtures that look alike involves the further statement that color comparisons remain valid if the intensity of all component lights is increased or decreased in the same proportion. This he shows is not quite correct for dichromatic systems like common cases of color blindness. By mixing light of wave-lengths 615 μ and 460 μ, a colorless mixture can be produced which remains colorless if their intensity is changed. A homogeneous light, however, which with a definite intensity of that mixture looks the same, becomes more yellow if the intensity is increased. To maintain the same color by such increase there must be a relative increase in the quantity of light of longer wave-length. The same thing is true of tri-chromatic systems. This is, however, harder to observe and is opposed to the results of Hering. It is best seen in mixtures of red, green and yellow reduced from middle intensity.

Tambroni and Algeni (Riv. Sperim. di Fren. XI) measured the duration of psychic reaction in the field of the space sense of the insane. The method of right and wrong cases was used in distinguishing whether one or two compass-points were applied. These observations, which require great patience and care, were made on four normal persons and four melancholics, dements, epileptics and maniacs each, making twenty-four patients in all. Two points required more time than one, and wrong judgments were longer yet. The average error, number of errors and time were also reduced by practice. These results with normal subjects were before known.

The effects of practice were observed only with maniacs and in part with epileptics. The aggregate results show that melancholics have the longest reaction times, and then come epileptics, dements and maniacs, and normal persons in descending series of times. That this scale or the numbers are typical it would at present be rash to affirm.

Tschis (Wjestnik. Psichiatrij, Bd. III) reports a study of the same problem with Flecheig's patients in Wundt's laboratory. Three cases of incipient dementia were psychometrically tested as to simple reaction time of choice, association and inference. With each subject an acceleration of the process of active apperception was demonstrated, whence Tschis concludes that morbid processes begin with a weakening of active perception. Every form of mental alienation, it is inferred, must begin with a reduction of the free creative function of will, for in this function the ego is determined by the entire conscious past.

Guicciardi and Cionini (Riv. Sperim. di Fren. XI) studied experimentally the effect of practice or memory, as they indifferently call it, on the duration of the following simple psychic processes, with tables for the successive days: simple reaction, the discrimination of touch on two points, the distinguishing of two spoken syllables with choice-reaction, the perception of three figures in predetermined order, reproduction of written letters, and word-association. The time beyond which no further reduction could be effected by practice was greater as the process was complicated. By very complex processes the longest time was generally *not* the first but perhaps the third reaction. After a pause of three weeks the reaction times were at first greatly increased, but very rapidly reached the previous minimum.

The results were not unknown before, the experimental process is not made very clear, and the theoretical introduction is very long and dull.

A prolonged and valuable study of the variability of the development of cerebral bloodvessels and their physiological and pathological significance, made by L. Löwenfeld, is reported in a late number of the *Arch. f. Psych.* The diameter of the basal vessels of the brain was measured in over 200 cases and compared with the weight of the normal brain. The relative variation was found so very great as to indicate that beside other factors, the nutrition of the brain is of great significance for its function. The sum of arterial capacity compared with 100 gr. of brain weight varied from 0.175 to 0.315 cm., age being a moderate factor in this variation. The left carotid was generally wider than the right. This variable has, in the discussion by the author which follows, great significance in explaining mental endowment, power of work, disposition to neuroses and psychoses, etc.

In a letter by Professor A. Pick to the editor of the *Neurolog. Centralblatt*, written Oct. 27, 1887, the statement of Prof. Steinbrügge that secondary sensations, or the fact that certain persons react with twofold sensation upon one simple sensation, were only known within the last few years (from which he is inclined to draw pathogenetic conclusions), is corrected by interesting citations from earlier

literature. In his jurisprudential psychology (1842), Friedreich cites the observation of a cultivated deaf-mute in whom music excited peculiar agitation in the feet and body which produced the most diverse moods. To these sensations, produced by different instruments, he gave color-names: trumpet, yellow; drum, red; organ, green, etc. Again, in the *Archiv ital. per le mal. nerv.*, 1865, says Pick, Berti describes an individual who, on looking at certain numerals, letters, etc., was impressed with imperative color concepts, and thinks it due to persistent association, and refers it to the field of Daltonism. This phenomenon seems to have been first named by Dr. Chevalier (Gaz. Méd. de Lyon, 1864) pseudocromastesia.

III.—ABNORMAL.

Dr. W. Stark (Zeitsch. f. Psychiatrie, 1887, Heft 2 and 3) recorded the weekly variations of weight in six periodic and six circular forms of psychosis. In ten of these cases each paroxysm, whether maniacal or depressive, is attended by a descent of the weight curve, and each interval by an ascent of the curve. Both changes were greatest near the beginning of the paroxysm or interval. Restlessness and reduction in the amount of food probably account for the decrease. A study of metabolic modifications during these psychic changes is strongly desiderated.

After describing briefly six cases gathered from literature of similar psychosis of twins (see also Galton's interesting chapter on the psychic peculiarities of twins), Dr. H. Euphrat (Zeitsch. f. Psycho., 1887, H. 2 and 3), adds an interesting account of two maiden women, alike in character, but physically and mentally different, both of whom, at the age of about 40, one two years later than the other, had very similar attacks of nervousness, hallucinations of vision, hearing, touch, somatic feeling, and delusional ideas. There was no hereditary predisposition save that the father died of delirium tremens. From this and the other six cases, Dr. Euphrat dissents from Ball's opinion that such cases are entirely due to anatomical likeness of brain structure, and thinks the similarity of the psychoses to be due to psychic contagion, induction, or infection.

In a paper presented before the psychological section of the British Medical Association, 1887, Dr. Hack Tuke would call many of the cases commonly designated as *folie-à-deux*, communicated insanity. This latter term should at least be applied to cases in which one member of a family becomes insane from over-work or distress for another insane member. Women are more liable to such contagion than men. Especially those delusional ideas that have some semblance of truth—notably delusions of persecution—are transferred. For this reason the mildly insane should not be cared for by their friends, especially if the latter are of nervous temperament. This class of cases need far more detailed study than they have yet received.

Dr. Battaglia, director of an insane asylum in Cairo, describes many experiments upon himself with different qualities of hashish (La Psichiatria, 1887). He produced a great variety of symptoms with great uniformity, but never the commonly reported euphoric

apathy. This feeling, as well as the vanishing of time and space, sexual excitement, hallucinations of vision and hearing, he ascribes to other drugs often mixed with hashish, which, if pure, is only soporific. Cannabism begins with a stupid staring expression, and passes to apathetic melancholy and dementia. The prodromal stages of paranoia predispose to this habit with the national apathetic tendencies of the Oriental character, as in America on the basis of a more excitable temperament they predispose to alcoholism. Total, as distinct from gradual, abstinence is on the whole the least straining method of cure.

Dr. Crothers, of Hartford, estimates that of the half million drunkards in this country, about ninety per cent die of diseases due to this habit, and about the same per cent inherit degenerative nervous systems. Drunkenness can never be successfully resisted so long as it is regarded as a vice or a crime. It is a disease, and the inebriate must be forced into quarantine and there be treated till he recovers. Society may demand that no acute drinker be allowed to become chronic and incapacitated for work, and to prevent this may treat the patient by isolation as if he had a contagious disease.

Dr. S. Tonnini presented his somewhat novel views upon secondary paranoia at the late congress of Italian physicians at Siena, where they were met with many objections. He now (Riv. Sperim. di Freniatr. 1887, XIII) defines it more fully as personally acquired by a previously sound person in distinction from primary paranoia, which he regards as the further development of an inherited neuropsychopathic degeneration. A degenerative state, if not proclivity of the brain, such as is often the inherited result of a psychosis in an ascendant, may thus in some cases be acquired in an individual experience. The inherited basis may indeed be bad, and even predisposing, but will not bring the individual to paranoia without a new impulse. Only in secondary cases does recovery or full-blown stupidity occur. The current view of secondary paranoia regards it as residual or sequent to more active delirium. Dr. Tonnini appends five cases which seem to conform to his definitions.

Dr. Rudolph Arndt describes a remarkable case of trophic disturbance due to violent psychic excitement (Deutsch. Med. Woch., 1887, No. 34). Albumen, hyaline cylinders, and epithelium from the urinary canal appeared in the urine, and the liver absorbed gall without any stoppage of the gall ducts. The author attempts to explain at length that psychic processes are only attendant if not incidental phenomena of metabolic and other physical processes. Therefore it is wrong to say the above symptoms were caused by fright, but they were rather due to the molecular-atomic processes set up by the shock, which, in reverberating through the system, affected the psychic organism first.

Edmond Grasset, in a very interesting doctoral thesis (Bordeaux, 1887) on alcoholic disturbances of cutaneous sensibility, based on very detailed tests on twelve subjects, distinguishes these symptoms as objective and subjective. The former are alphalgesia (painfulness of touch), thermo-analgesia, electro-analgesia, and especially hyperæsthesia, analgesia from pricks. These disturbances are

distributed irregularly in spots and do not correspond with distinct nerve areas, and change both spontaneously and from external cause, for, strange to say, dermal and organic reflexes do not seem to be modified by these areas. Subjective disturbances consist of darting pains, formication, etc., generally in the limbs. Besides these, disturbance of sensibility in deeper tissues and internal organs and organs of sense, sometimes somewhat resembling hysteric symptoms, is common. The cause may be peripheral neuritis, lesion of the internal capsule, and especially of the pons and crura.

Seppelli (Revist. Sperim. di Freniatr., 1887, XII) has studied the blood of 104 male and 96 female lunatics, with the apparatus of Hayem and Nachet, to determine how the number of red blood corpuscles compared with that in the blood of normal subjects. In the latter there are about five millions per cubic millimetre in men and four and a half in women. Seppelli found this number reduced over fifty-two per cent in men and over sixty-three per cent in women. In pellagra this reduction was greatest, in melancholia next, and in mania least. The proportion of white to red corpuscles (normally 1.650 to 1.1300) was not greatly affected, though the figures indicate that it was rather less than more. The quantity of haemoglobin (tested by the chromocytometer of Bizzozero) was also much reduced. Both these reductions seemed to this indefatigable investigator greatest near the beginning of the psychoses, and both abnormalities were greater in men than in women.

In the *Revue de Médecine* Féré gives an interesting case of a rich merchant of 37, who in 1886 began to have "absences," in which he would suddenly stop in the midst of any business and stand motionless and smiling, sometimes for fifteen minutes. He at length consulted a physician, to whom he told the following history: As a boy, he took all injuries very hard and would brood over them for hours in solitude. He used to lapse during these brooding fits into revery and castle-building, at first ephemeral and changing, but gradually permanent. He played many roles, according to his mood—soldier, statesman, scholar. After college, when business and domestic cares came, these reveries diminished till gradual insomnia brought back his musings, which now assumed definite form and took complete possession of him. For the last four years his reveries had slowly built a pavilion at Chaville with a pretty garden. By gradual additions the former became a mansion and the latter a park. Conservatories, stables, servants, and finally a beautiful woman came. Two loveiy children crowned a joy that would have been complete but that his union to this imaginary woman (who was so real that he had grown entirely cold to his wife and almost forgot the existence of his children) was not legally his wife. These hallucinations were of visual origin and yielded to tonic treatment. Of similar nature, perhaps, were Mahomet's reveries during the years of cave-life, and Jeanne d'Arc's mystic day-dreams among the hills of Lorraine, and many other visionaries who have become honest victims of their own fancies. This class of cases must not be identified with those described in Dr. Clark's book on visions, which begin in distinct optical delusions at first recognized as such.

Pseudo-hallucinations, as conceived by Kandinsky in his very

valuable book of that name, are described as perverted memory- and fancy-concepts as vivid as real hallucinations, but lacking the sense of objective reality without the patient, *e. g.* words heard inwardly with the spiritual ear without a realization of their subjective origin. This conception J. Hoppe opposes (Jahrb. f. Psychiatrie, VII, 1 und 2), as it involves laying too great stress on the mere subjective appearance of externality, instead of considering the state of the peripheral nerves of the sense involved. Kandinsky's carefully studied cases are criticised at length, and the phenomena referred to real hallucination due to either entoptic or subcortical material or to concepts. Inner hearing is also said to be often attended by unconscious and faint articulating movements.

The *Journal of Mental Science* lately contained an article on facial blemishes as a cause of melancholia, in which it was said that at about the age of forty, single women sometimes conclude they are not attractive, and magnify some real or fancied defect; and married women, fearing to lose their husband's affection, sometimes grow self-conscious or jealous. Hair on the face, wrinkles, fatness or leanness, or scars, may cause depression.

Multiple paramyoclonus, involving clonic and even tetanic contractions—not fibrillar but of entire muscles—of the muscles of limbs, neck, back, is hard to distinguish from convulsive tic, or from chorea major, save that it is often symmetrical and rarely affects the face. In a case lately reported, even the muscles of the uterus, heart, diaphragm, bowels, etc., were affected with the characteristic twitches. A neuropathic basis and shock or psychalgia are the etiological moments.

The *Neurological Review* calls attention to "the astonishing apathy that exists as a whole in regard to the importance of a knowledge of the nervous system in the daily work of every member of the medical profession." The writer reminds us that the nerves penetrate and to a greater or less extent control every organ and tissue of the body and every physiological function, and concludes that it is absurd to leave neuro-psychic matters to specialists in medicine, as is commonly done in practice.